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T R E A T I S E
ON THE
VENOM OF THE VIPER;
ON THE
AMERICAN POISONS;
AND ON THE
CHERRY LAUREL,
AND SOME OTHER
VEGETABLE POISONS.

TO WHICH ARE ANNEXED,

OBSERVATIONS ON THE PRIMITIVE STRUCTURE OF
THE ANIMAL BODY; DIFFERENT EXPERIMENTS ON
THE REPRODUCTION OF THE NERVES; AND A DE-
SCRIPTION OF A NEW CANAL OF THE EYE.

WITH TEN DESCRIPTIVE PLATES.

TRANSLATED FROM THE ORIGINAL FRENCH OF

F E L I X F O N T A N A,

NATURALIST TO HIS ROYAL HIGHNESS THE GRAND DUKE OF TUSCANY,
AND DIRECTOR OF HIS CABINET OF NATURAL HISTORY.

BY JOSEPH SKINNER,

NAVY SURGEON, AND MEMBER OF THE CORPORATION OF SURGEONS
OF LONDON.

IN TWO VOLUMES.

V O L. II.

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C O N T E N T S

OF THE

SECOND VOLUME.

PART THE FOURTH.

CHAP. I.— <i>Examination of the remedies employed against the bite of the viper</i>	I
CHAP. II.— <i>If the bite of the viper is naturally mortal to man</i>	34
————— <i>Treatise on the American poison, called Ticunas</i>	96
————— <i>First Tract on the Cherry-laurel</i>	143
————— <i>Second Tract on the Cherry-laurel</i>	156
————— <i>Experiments on several vegetable substances</i>	181
————— <i>Considerations on the nerves in diseases</i>	186
————— <i>Experiments made at London in 1778, and 1779, on the reproduction of the nerves</i>	203
CHAP.	

CHAP. II.— <i>Observations on the primitive structure of the animal body</i>	-	-	215
— on the structure of the nerves	-	-	<i>ibid</i>
— on the structure of the brain	-	-	241
— on the structure of the tendons	-	-	256
— <i>Reflections on the motions of the muscles</i>	-	-	277
— <i>Microscopical errors, and consequences deduced from microscopical observations on several excrementitious, and other parts of the body; on animal, mineral, and vegetable substances; and on fossils, metals, &c.</i>	-	-	284
— <i>Letter to Mr. Adolphus Murray, Professor of Anatomy at Upsal, in the year 1778, on the subject of the discovery of a new canal of the eye</i>	-	-	310
SUPPLEMENT	-	-	315
INDEX.			
EXPLANATION OF THE PLATES.			

E R R A T A,

Page 17, line 20, for *he*, read *it*.

— 98, — 11, for *banks of the Amazons*, read *banks of the
river of the Amazons*.

— 201, — 3, for *Sir Robert Pringle*, read *Sir John
Pringle*.

— 294, — 24 and 25, for *lamina*, read *laminæ*.

— 312, — 26, for *which is wrapped in its substance*, read
in the substance of which it is infolded.

PHILOSOPHICAL RESEARCHES
INTO THE
VENOM OF THE VIPER.

P A R T IV.

C H A P. I.

*Examination of the Remedies employed against the
bite of the Viper.*

AFTER having examined the quality of the viper's venom, and being much better acquainted than hitherto with the nature of this poison, it appeared to be no longer difficult to find a remedy for it. This is the usual way of reasoning, and is the source of numberless remedies, which succeed each other, and are at length found by experience to be hurtful, or useless at least. The fluid volatile alkali owes its greatest repute to the re-

B received

ceived opinion that the nature of the viper's venom was discovered. Jussieu, from the authority of Mead, deemed it an acid, and this was sufficient to give the volatile alkali the reputation of being a true antidote against it.

The authors who succeeded Jussieu copied from each other. They adopted the remedy, and its mode of action, and they found it a good one, because the bite of the viper is not always fatal in its consequences. For my part, I am of opinion, that even when the nature of a poison is well known, and the effects it produces on animals, we may very easily be at a loss for its remedy. Nothing is less difficult to conceive than this, if we reflect how very ignorant we still are as to the animal machine, and how much we are in the dark and in uncertainty, as to the qualities, or the virtues of bodies.

Be that as it may, it is clear that the authority of certain writers has brought into fashion a greater number of remedies, than has been introduced by the fallacious experiments of others, or the scarcity and price of the remedies themselves. The bezoar, for instance, the unicorn, and rhinoceros, are of the latter description. Were we to examine by the rigid touchstone of experiment, the long list of remedies, to how few would they not be reduced? It is on this account that the best collection of receipes is invariably the shortest.

We have already seen above, in what estimation the fluid volatile alkali, regarded as a specifick,

should be held. All my experiments shew it to be totally useless, even when taken inwardly. It does not appear that it can have the smallest effect when applied to the part bitten. It is almost needless to observe, that the volatile alkali, whether given internally, or applied to the part, can in no way serve to correct the acidity of the venom, since the venom is certainly not acid; whence it is that these boasted qualities of an alkalescent principle, this neutralization of salts, are mere fancies and errors produced by ill experiments. I believe besides, that even though the venom of the viper should be acid, and destructive to animals as an acid principle, we ought to expect little or nothing from the volatile alkali applied externally. To the end that the volatile alkali should saturate the acid of the venom, it would be necessary that it should find its way into the part bitten, and should there mix and unite itself with it. It appears to me, that in general, the volatile alkali does not reach the muscles through the skin, at the place where the poison has penetrated. 'Tis at least what I observed in those animals that have a compact skin, like that of the human body.

*Experiments on the effects of the Volatile Alkali upon
the bite of the Viper.*

I cut a piece of the skin under the belly of a guineapig, the incision being in the shape of

a parellelogram, three sides of which were separated from the animal, the fourth still hanging to it. I pierced the cut skin with the dried teeth of a viper, which penetrated it through and through. Under the hairy side of this skin, thus prepared, I held a bottle filled with volatile alkali, and having a mouth of four lines diameter, but could never perceive that any smell was communicated to the inside of the skin, how long soever I kept it over the bottle, and however great the strength of the volatile alkali, which was active to a degree.

I repeated this experiment on rabbits, that have a skin still thinner. The event was the same: no smell could be perceived through the skin.

I moistened the inner part of the skin of a guinea-pig, which I had previously pierced with the dried teeth of a viper, as before, with a little diluted nitrous acid. Whatever quantity of the volatile alkali I threw upon the outer part of the skin, the nitrous acid never appeared to be saturated, or in the smallest degree weakened. At another time I moistened the skin of another guineapig, prepared as above, with a dissolution of copper in well diluted nitrous acid, and kept the outer part of the skin wet with the fluid volatile alkali: the dissolution of copper did not change its colour to blue.

It is then certain, that in general the volatile alkali does not penetrate through the compact skin of a quadruped; the reason of which is, that the tooth of a viper being very small, does no more
than

than displace the parts a little, and in proportion as it is withdrawn, the skin, by its elasticity, recovers its former situation, and closes the hole. It on this account frequently happens, that the bite of a viper does not cause the animal to bleed. If a considerable vessel has been punctured by the tooth, the blood issues out, coagulates, and prevents the entry of other substances.

The human skin is thicker than that of rabbits and guineapigs; it is of a very close contexture, and very elastick. If the fluid volatile alkali were a true specifick, whenever it could unite itself with the viper's venom, it would be inefficacious against the bite of a viper in man, or could at best serve only for bites in the skin altogether superficial, which are never to be dreaded in any quadruped, however small, and much less in man.

But if the volatile alkali is usefess when applied externally to the part, from the difficulty it finds to mix with the poison; why may it not be useful, if we contrive to introduce it into the parts bitten? greater or smaller incisions may be made in animals, and the volatile alkali conveyed by this means to the wounded muscles. Will it in this case be a specifick? Will it at least be useful?

To fully elucidate this I made the following experiments.

I had several animals, such as hens, rabbits, guineapigs, &c. bit in the leg, and some minutes after made deep and extensive incisions into the wounded parts. I washed these incisions with pure

volatile alkali, and covered the legs with linen bandages. I got ready an equal number of animals of the same size, and of the same kinds, to serve as a comparison. These were likewise bit in the leg, but I neither made incisions, nor applied to them the volatile alkali. The result of twenty-four experiments was not favourable to this medicine applied to the incisions, and the violence of the disease was even more considerable in the former than in the latter.

I cannot here pass over an experiment which was suggested to me by the *Duc de Chaulnes*, and which I made a little time after, with his assistance, on a pigeon, in the presence of a celebrated physician and chymist, Monsieur Darcet. I mixed together equal proportions of venom and volatile alkali, and introduced a part of this mixture into the pectoral muscles. The pigeon died at the end of eleven minutes. As I had some suspicion that in the operation, I had penetrated into the cavity of the thorax, I thought proper to repeat the experiment on other animals. Besides this, I varied the quantities of the venom, and of the volatile alkali, and likewise made use of fluid volatile alkalis, prepared without lime. Of six pigeons who were poisoned in the breast, and six others in the leg, not one recovered, and all died in a very short space.

I put into a small glass, three drops of viper's venom, and twelve drops of fluid volatile alkali. After having mixed these two fluids together, I
put

put half a drop of them on the divided fibres of one of the muscles of a pigeon. It died at the end of thirty hours, with the symptoms of poison, but in a moderate degree.

I repeated this experiment on another pigeon, the muscles of which I had laid bare, and wounded in several places. Into these muscles I introduced a small piece of wood well covered with the venom taken from the glass. The pigeon survived, although the symptoms of poison discovered themselves.

I repeated this second experiment on another pigeon, and introduced as above, the piece of wood covered with the venom. The animal lived, and had scarce any ailment.

I made a fresh experiment with the piece of wood dipped in the venom, and in less than an hour the pigeon died.

The little uniformity in the result of these experiments made me suspect, that the poison had not well communicated itself to the muscles, and that the use of the piece of wood was not the best way to communicate the disease. Some succeeding experiments confirmed me in this suspicion.

I formed then the idea of introducing into the muscles of pigeons, the venom from the small glass, by means of a thread repeatedly doubled. I passed the threads well rubbed over with this poisonous mixture, through the muscles, and left them there. Six pigeons who underwent this ex-

periment, all died in the space of thirty-seven minutes.

It is, however, possible, that the fluid volatile alkali contracts the vessels in such a way, that the venom cannot be easily absorbed; but be that as it will, we see clearly that when it is well applied, it is mortal as before, and that the volatile alkali does not diminish its activity.

These experiments not only demonstrate the absolute inutility of the volatile alkali against the bite of the viper, when applied externally; but they at the same time prove still further, that it cannot have a direct and specifick operation, when it is even taken internally. If the venom of the viper preserves all its malignant qualities when in immediate union with the volatile alkali, how can it ever be deprived of them, by meeting with it, when it is itself united in the animal, with an immense quantity of fluids, and dispersed in so many parts?

*Experiments on the inefficacy of different Substances
against the bite of the Viper.*

The same experiments may serve equally to exclude from the number of specificks, the many boasted remedies against the venom of the viper. I united a great number of substances with this poison, but did not observe after all, that it lost its hurtful qualities. I mixed it with acids, with alkalies,

kalies, with neutral salts, with oils; but it continued to destroy the animals, whenever it had found its way into their wounds.

I likewise made direct experiments on these substances, to be more certain of their inutility. I shall not here enter into a detail of these experiments, because it would be too long, and because I think it of little consequence; it will be sufficient for me to say in a general way, that I applied them to the parts bitten by the viper; even making incisions, to give the venom a freer communication. In these experiments I tried oil of vitriol, the nitrous acid, the phosphorick acid, and the mineral acid, (*l'acide spathique*,) and I found them all to be at least useless. Alkaline salts, caustick and not caustick, whether mineral, vegetable, or animal, had the same effect. I was still more attentive to the neutral salts, and above all to sea salt, which several esteem a good remedy, but which I also found useless. As to oils in general, and especially that of turpentine, they appeared to me to be of some real use. The best mode of application is, to foment for a long time the part of the animal that has been bitten, in the oil made extremely warm. Some guineapigs that according to the probabilities that resulted from my experiments on these animals, would have died, were perfectly cured by it. It is very true that they had been bit by a single Viper, and only once, and that two of the feet bitten, had lost the skin,

skin, and were in part crippled, which was probably occasioned by the too great heat of the oil.

I made repeated experiments by dipping the part that had been bit in various fluids. The plunging it into very warm water, and keeping it there, appeared to me to be truly advantageous. The pain was evidently lessened, the inflammation abated, and the colour less livid and changed. I found the same effects from lime water, and from water mixed with common salt, and other saline substances. Although the immersion is neither a specifick, nor a certain remedy, it is always attended with a greater or lesser degree of advantage, which I think is in these cases due to the mere fomentation with warm water.

I observed in the course of my experiments, that dogs and cats recovered with a facility proportioned to the violence of their vomitings; and wishing to follow this indication of nature, I made a great number of experiments on dogs. Here I was very often led to believe the emetick a good remedy, as I sometimes met with seven or eight cases which terminated in the same way, and were altogether favourable. The emetick I made use of was the stibiatic tartar, which I gave in water, in different doses, and at different periods. The result of some of these experiments contradicted that of others, but several of them were very favourable and uniform. Amongst a great number of other trials, I had a dozen dogs bit in the leg, each by three vipers, and by each repeatedly.

edly. To fix of them I gave emetick tartar, to the others nothing. All of the first recovered; four of the others died in less than three days. I cannot therefore take upon me to determine the tartar emetick entirely usefess, but it is certainly no specifick, no assured remedy.

I wished to try cantharides, not because I was strongly induced to believe them good against the bite of the viper, but only because I wished to see how an active substance, and that too in some degree poisonous, would act on an animal attacked with the symptoms of poison.

I applied the cantharides to the bitten part, and likewise caused them to be taken internally. I soon perceived that, applied to the part, they were evidently injurious, and that there was a speedy disposition to gangrene and sphacelus. To introduce them still better, I made incisions.

Those taken internally furnished me, like the emetick, with equivocal conclusions, in proportion to the uncertainty of which, I multiplied my experiments, and was at length assured that the cantharides, are neither a specifick, nor an efficacious remedy, although, I can neither declare them hurtful nor usefess.

I conceived greater hopes from the bark, which is avowedly a powerful antiseptick, and very useful in cases of gangrene. For the short time the complaint lasts, the viper's poison produces a true local gangrene, and the bark was therefore promising. I began my experiments with the simple powder,

der, which I threw on the part bitten, into which I had made several incisions. Not-perceiving that it had a certain good effect, I had recourse to the bark in infusion. I bathed the wounded part of the animal lengthways, sometimes keeping it for a long space entirely plunged in the warm infusion. At other times, I plunged it repeatedly in, but it was all in vain: I could never assure myself that it had a real and constant advantage, although I cannot condemn it as entirely useless.

It was necessary to make an incredible number of experiments, before I could determine the little certainty of emeticks, cantharides, and bark, opposed to the bite of the viper: these experiments are besides extremely inconvenient, as one operates principally upon dogs, and they are for the most part of very long duration. A dog when he survives, frequently remains sick ten, fifteen, or even twenty days.

I wished to try besides, if scarifications more or less deep, and the actual cautery, were certain remedies. The conclusions I obtained, which were very numerous, were not favourable to these two methods, which are however proposed by authors with great confidence. It appeared to me on the contrary that the scarifications, very far from being useful, rather did harm; the part bitten, and afterwards scarified, being the more disposed to gangrene. In a word, neither the scarifications nor the cautery had any useful effect.

It

It remained for me to try two other remedies which many celebrated physicians prefer to many others.---The theriaca and the fat of the viper itself.

I employed the theriaca by laying it on the part, which had been bit, and afterwards scarified. I renewed it several times, and kept the part well covered with picces of linen spread with the same. I likewise gave it internally, but all without effect: it did not appear to me to be of any use in diminishing the animal's complaint.

Mead in his treatise on poisons, speaks of a remedy which was reckoned in his time, a real specific against the bite of the viper. He says that the viper catchers in England used it with so much confidence, that they were no more afraid of the bite of a viper than of a common prick.

Mead contrived to ferret out this remedy, which was yet a secret. He discovered that it was the fat of the viper itself, which they rubbed over the bitten part. To be still surer of the efficacy of this remedy, he had the nose of a dog bit by a viper, and applied the fat to it. The animal recovered, and on repeating the experiment, the result was the same. Being thus assured of the efficacy of the remedy, he sat about explaining philosophically how it ought to correct the action of the poison. He found that the glutinous corpuscles of the viper's fat were calculated to enclose the volatile salts of the poison, and so to prevent their uniting into chrystalline salts,
to

to which the poison owes its force of activity.

Mead's principal mistake consists in having supposed that the bite of a viper on a dog's nose was absolutely mortal. On another hand what dependance ought we to place on two experiments only? It has been seen above, how different the consequences are when the circumstances even appear the same, and how little trust should be placed even in uniform consequences, provided the number of experiments be not very great.

Bites of the viper on the nose, are less dangerous than in all other parts of the body. If Mead had multiplied his experiments more, if he had varied them as he should have done, he would not have been deceived, or would have been sooner convinced of his error. From hence principally arises the slowness in progress of the science of natural philosophy, and this is the source of an infinite number of mistakes which continue to disfigure medicine, and to prevent its advancement.

I have likewise tried electricity against the bite of the viper, and have not only found it useless, but it has even appeared to be hurtful. It is at least certain that in the animals to which I applied it, the complaint became more violent, and that they died sooner. In many, I threw sparks from the conductor, on the part bitten; and in others drew sparks from the part, keeping the animal fastened to the conductor: in both ways I found electricity more hurtful than beneficial.

Applica-

Application of Leeches to the Bite of the Viper.

I had a pigeon bit in the leg by a viper, and instantly applied to the part three leeches which fastened very well. In twenty minutes it died, the leeches being swoln with the blood they had sucked from it.

I repeated the same experiment on two other pigeons, which were but just bit when the leeches were applied. They both died in eighteen minutes.

The sucking the parts bit by the Viper.

I was desirous to see if by sucking the part immediately after its being bit, the poison could be prevented from diffusing itself. I met with a person who made no difficulty of sucking it.

I had the bites of two pigeons sucked, without dilating them, and those of two others, after having dilated the holes made by the teeth, and begun to scarify. All four died in less than twenty-seven minutes.

The same experiments terminated the same way with quadrupeds; after which I am not afraid to pronounce that neither suction by the mouth, nor the application of leeches, is a certain remedy against the bite of the viper.

I shall not dwell on several other methods I have tried against the venom of this animal, and which
I have

I have found more or less useless, and sometimes hurtful. I have applied several earths, chymical preparations, and vegetable substances, to the bitten part, and have sometimes even given them inwardly to the animals. I think it useless to insert a catalogue of superfluous remedies.

On the utility of amputating the Limbs bit by the Viper.

We have already seen that the action of the viper's venom is not instantaneous ; that it requires a certain time for its effects to be perceived in the bitten parts ; and that the external malady does not communicate itself suddenly to the animal. We have also seen, that if the part bit by the viper be suddenly amputated, the animal survives. All these experiments together supply a certain remedy against the bite of the viper, when one can practice it with facility. It is natural to suppose, that by lopping off the diseased parts, the life of the animal may be saved ; but the amputation ought not to be much retarded, because it is at least certain, that the sooner it is performed the surer its effects. In pigeons it begins to be even fatal at the end of fifteen seconds, at which time the internal malady is communicated, which the amputation encreases, and hastens death, instead of diminishing one and retarding the other, as I have been satisfied by several experiments.

Before

Before I examined into the advantages of amputations on animals bit by the viper, I wished to see whether the internal malady would be communicated in a sensible way, and so as even to occasion death in other animals, in the same space of time as in pigeons. It was necessary to try it on animals that die with much greater difficulty than these last, but who would die to a certainty, and in a space not too distant from the introduction of the poison. I made choice of very small guineapigs, because I knew by experience, that they had all these qualities.

I had a guineapig bit several times at the extremity of the foot, which at the end of twenty seconds I cut off betwixt the tarsus and the tibia. The animal lived, and seemed to have no other complaint than that caused by the operation.

I had another guineapig bit repeatedly at the extremity of the foot, by a viper, and at the end of forty seconds cut off the leg as above. He recovered in the same way with the former.

A third guineapig received several bites in the foot, from a viper, a minute after which I cut off its leg. It recovered as well as the others.

I had another guineapig bit repeatedly by a viper, in the foot, which in eighty seconds I cut off; this one likewise recovered.

I had another guineapig bit repeatedly in the foot by a viper, and two minutes after cut off its leg; it recovered likewise.

I had another guineapig bit several times by a viper, in the foot, which at the end of three minutes I cut off: this one recovered too.

I had another guineapig bit several times by a viper, at the extremity off the foot, and at the end of four minutes cut off its leg: it died three hours after, having a lividness of the muscles of the leg, and the auricles and heart filled with clotted blood.

I had another guineapig bit repeatedly in the foot by a viper, and at the end of four minutes cut off its leg: it recovered.

It must be observed, that the feet amputated at the end of three or four minutes, have unequivocal signs of local malady; these signs are even observed before, although with more difficulty, are not so certain, and do not exist always.

Neither of the guineapigs bitten in the feet, and mutilated in less than three minutes, died; but of the two mutilated at the end of four minutes one died, and the other survived. There are even here then, as well as in many other cases we have seen above, circumstances in which the bite of the viper produces greater or lesser effects; but what is more important, and deserves all our attention, is, that the internal malady is not communicated to the animal till very late, in comparison to that in the cases of the pigeons, or more properly, that it does not become mortal till after a long time, and that the cutting off the part bitten may be made

with all possible advantage and safety, within the limits of a much greater time.

But let us continue our experiments, the number of which has been too small to supply us with certain conclusions.

I had a guineapig bit several times in the foot by a viper, and cut off its leg at the end of four minutes: it recovered.

I had another one bit repeatedly in the foot by a viper, and cut off its leg at the end of five minutes: it recovered.

I had another guineapig bit in this way in the foot, and at the end of six minutes cut off its leg; it died ten minutes after.

I had three guineapigs bit in the foot by a viper, each one several times, and in four minutes cut from each a leg: they all recovered.

I had three others bit in the foot in the same manner, and in five minutes cut off a leg from each of them: they all three recovered.

I had three others bit in the same way, and in six minutes performed the operation: one only recovered.

I had three others bit as above, and in ten minutes cut off a leg from each: they all died.

From all these experiments it appears that this deduction may be made, that every thing is to be expected from the amputation of the leg, if performed on guineapigs before six minutes are elapsed after their being bit by the viper.

It is natural to suppose, that in larger animals the amputation may be made much later still than six minutes, and experience has demonstrated it in very large rabbits; but we may stumble on another inconvenience which confines this method very much: pigeons are not endangered by the amputation of the leg; small guineapigs bear that of the extremity of the foot, but not always that of the leg; larger animals are more apt to die when a great part of them, such as the leg, is lopped off; such an operation in these cases is not only useless but dangerous.

It does not, however, follow that amputation, even in large animals, may not be useful against the bite of the viper; in general it is so when the animal bears it easily, provided it be done at a convenient time. As amputation may be very useful in a great number of cases, I thought it incumbent on me to make experiments, and to vary them several ways, on different animals.

Rabbits and Dogs that had their ears bit and cut off.

I had a rabbit's ear bit a single time by a viper, and in thirty seconds cut it off six lines below the part bitten. The animal bled a great deal, but did not die, nor even appear more disordered than another rabbit, the ear of which I cut off, without having it bit.

I had

I had a second rabbit bit several times by a viper, and a minute after cut off the two ears, six lines below the bite. It recovered without any symptom of poison.

I had a third rabbit bit in both ears, by two vipers, each of them repeating its bites, and in two minutes cut off each ear, eight lines below the parts bitten; it recovered as well as the other two.

I had two other rabbits bit several times in both ears, each by two vipers. At the end of six minutes I cut off the ears, eight lines below the bites. Both recovered, without any intermediate symptom of poison.

I had a small dog bit in the ear, which in a minute I cut off six lines below the part bitten. The dog recovered without any other symptoms than those common to such an operation.

In the same way I had another small dog bit repeatedly in the ear by two vipers, and in six minutes cut off the part. It recovered without having had any other symptoms than those resulting from the operation.

Again, I caused a young and small dog to be bit in both ears by two vipers, and by each repeatedly. In twenty minutes I cut off the parts. It recovered without having any symptoms of poison.

I repeated this last experiment on two other dogs, and it succeeded equally, since both of them lived. 'Tis very true they were much disordered, but not

more so than dogs are used to be when their ears are cut off, without having been bit.

As it is not common for either rabbits or dogs to die when bit in the ears, above all, if they are somewhat large, the experiments on these animals only serve to prove that the effects, at least local, do not subsist after the bitten parts are cut off.

Animals that have received bites on the Skin, into which incisions have been afterwards made.

I had a very small guineapig bit repeatedly by a viper on the skin of the back, and to prevent the viper's wounding the muscles, I kept the skin raised with pincers, the teeth piercing it through and through. I held it thus for four minutes, and then dissected it away in such a manner that none of it remained for several lines round the part bitten. The animal recovered in twenty-four hours, the incision in its skin being covered with an eschar. It eat as usual, and appeared to suffer nothing except what was simply caused by the incision, as I assured myself by preparing another guineapig, to serve as a comparison, and which recovered in the same time, but had not been bit by a viper.

I had another guineapig bit several times on the skin by a viper; I kept the skin raised for four minutes after its being bit, and at length cut it. It had already the marks of poison upon it, that is
to

to say, livid and black spots, and these spots extended for some space round the part bitten.

I had another guineapig bit repeatedly by a viper on the skin, which in four minutes I dissected away. The creature recovered without any symptoms of poison.

I had three rabbits bit as above, but made no incisions; they all died, one in sixteen hours, another in twenty-six, and the third in thirty-two. In this last the flesh bitten was gangrened internally, and the cellular membrane, the pectoral muscles, and those of the abdomen, were filled with black and extravasated blood. The two other rabbits had likewise evident marks of disease and gangrene, but in a lesser degree.

I had two small guineapigs bit as usual on the skin, and in twenty minutes made the incision. Both recovered very well.

As the bite of the viper is usually mortal in these animals, even when it does not penetrate beneath the skin, the incision into the part bitten becomes a sure remedy for them against its venom. I repeated the same experiments on dogs and rabbits, and the result was the same. The cure is certain, and the local complaint avoided, as likewise the internal one, in a great measure at least, although the amputation of the parts bitten be made much later.

Combs and Gills of Fowls bit, and afterwards cut off.

It has been seen above that the bite of a viper in a fowl's comb does no injury to the comb, but a great deal to the gills. This fact is not the less true for being singular, and is the result of many uniform and constant experiments.

As the venomous effects do not discover themselves in the comb, but in the gills, which are attacked with a complaint generally fatal to the hen, it was natural to suspect that the gills being cut off, the animal would recover perfectly.

I had a fowl bit, then, and that repeatedly, in the comb, by a viper, and in twenty seconds cut off the gills.

It recovered without any symptom of illness, continuing to eat and drink as usual.

I had the comb of another fowl bit repeatedly by a fresh viper, and in forty seconds, cut off the gills. It did not appear to have any complaint.

I had a fowl's comb bit several times as above, by two vipers, and in sixty seconds cut off the gills. It recovered without any ailment.

I had the combs of three fowl's bit repeatedly by two vipers, and cut them off, one in four minutes, another in eight, and the third in ten. They all recovered, the gills of the third were diseased at the end of the ninth minute.

It has been seen that when the gills, and not the combs, are bit by the viper, the complaint does
not

not find its way to the combs, but remains in the gills, is more dangerous than if the viper had bit the comb; and is usually fatal.

I had a fowl's gills bit several times by a viper, and cut them off in twenty seconds. It recovered without any apparent illness.

I had the gills of another fowl bit repeatedly by a viper, and cut them off in sixty seconds. It recovered, without any external symptoms of complaint.

I had three other fowls bit repeatedly in the gills by a viper, and in three minutes cut off the parts. The fowls did well, without betraying any ailment.

I had the gills of three other fowls bit in the same way, each by two vipers, and that repeatedly, and cut them off in four, six, and eight minutes. The fowls all recovered, without any symptom of poison, as if the gills had not been bit by the viper, but only cut off.

All the experiments hitherto made, tend in themselves to give the most flattering hopes that a remedy more easy, more universal, and less painful than amputation, may be found against the bite of the viper.

It has been seen that the nerve does not serve to communicate the disease of the venom to the animal; that this disease communicates itself by the blood; and that wounds of the skin, venomous, but superficial, are attended with no dangerous consequences. The two first truths point out with certainty

tainty that to prevent the complaint from communicating itself to the animal, it is sufficient to stop the circulation of the blood; the third demonstrates that it is not necessary to stop it totally, and in the very small vessels. Nothing, as I see, can be more conformable to the theory of the poison, and its modes of acting on the human body.

This great and useful truth ought to be founded on a number of incontrovertible experiments. I thought that the pigeon, of all animals, would furnish me with the least equivocal proofs, and therefore preferred it to all others. I knew that the bite of the viper is certainly mortal to it; that it dies in a few minutes; and that an imperceptible quantity of poison is capable of killing it in a short time. A single bite of the viper is capable of introducing as much poison into a pigeon, as would kill 200 of them.

I had a pigeon bit once in the leg by a viper, the part having been previously tied with a silk ribbon, just above the joint. The symptoms of local malady appeared very soon in the leg: in four hours it was quite livid and swelled beneath the ligature; but all above remained in its natural state. I took off the ligature, and a short time after observed that the leg was less swelled and livid. In ten hours its colour was almost natural, and it was but little swelled. In twenty-two hours nothing remained but a few discoloured spots at the place where the teeth had entered. In sixty hours there was a bluish cast over it. In three days it was perfectly sound.

I tied

I tied a ribbon about a pigeon's leg, and had it bit several times by a viper. In ten hours the leg was swelled and quite lived, discharging in several places a blackish humour. I removed the ligature. In twenty-two hours the leg was swelled as before, and black as a coal. In forty hours, all the muscles seemed to border upon sphacelus. In three days the leg was less swelled, and discharged less matter. In five it seemed in a healing state. In seven it had in a great measure regained its natural colour. The animal recovered in ten days.

I repeated this experiment on four other pigeons, but fearing that the ligature in the former one had been too tight, and had in a degree increased the local malady, I made the ligature much looser. Neither of the pigeons died, but their legs were somewhat swelled and livid. In ten hours I took off the ligatures. Two of the pigeons recovered the fifth day, the other two on the sixth.

It is an experienced truth, then, that the ligature made in the part bit by a viper, prevents the complaint from communicating itself to the animal, no internal disease supervening, during the time it remains on. It is likewise an experienced truth, and equally important, that at the end of a certain time, the venom does not communicate itself to the system.

Although it be true, as it indeed seems very probable, that on taking off the ligature, the venom is partly absorbed by the vessels, and carried with the blood into the circulation, it is however observed
that

that it is no longer in a state of poison, and capable of killing the animal. It is known that the smallest possible quantity of venom kills a pigeon in a few minutes, and experience shows, that none of them die when the ligature is made, although taken off at the end of a certain time.

Besides, it is not difficult to conceive that when once the venom has produced its ordinary effect on the blood, and on the parts bit by the viper, it ceases to be hurtful. Most bodies operate in this way, and the viper's venom may very possibly decompose itself in producing the local malady, and unite with the blood: but it requires a certain time to be brought into this state, and to be rendered inactive and innocent. In the cases cited above, the ligature was left on for ten hours. 'Tis very true, that all this seems contradicted by the experiments I have myself made on the venom, which, when mixed with the blood, does not on that account cease to be a poison. We have seen besides, that the venomous symptoms excited in the muscles of a pigeon's leg, communicate themselves very readily to the bared muscles of the leg of another pigeon, if they are placed for some time in contact with each other. But in all these cases, the trial has been made a few minutes after the pigeons were bit by the vipers, or after the venom had united itself with the blood. To determine after what time the ligature may be removed without danger, I made the following experiments.

I had

I had a pigeon bit in the leg by a viper, and in twenty seconds applied a ligature. In four hours the leg was swelled and livid, and discharged on all sides a blackish humour. I now removed the ligature, the parts above which were in a natural state. In ten hours the leg was less swelled and almost of its usual colour; there was, however, a degree of swelling above the ligature. In twenty-two hours the leg was scarcely swelled, although still a little livid; but the part above the ligature was both livid and swelled. In sixty hours there was scarce any mark of ailment, and on the fourth day the pigeon seemed perfectly recovered.

I had a pigeon bit in the leg by a viper, and in sixty seconds made a ligature. It died in three quarters of an hour, its leg having had a livid appearance before it was bound.

I bound the leg of a pigeon at least as tight as in any of the cases related above; in a short time it swelled, but not considerably; in seven hours it was more swelled, but with scarce any discoloration. On taking off the ligature at the end of ten hours, the tumour in the leg speedily subsided, but in return it swelled a little above the ligature. In twenty-two hours it was scarcely swelled or discoloured. In thirty hours the pigeon was perfectly recovered.

I had a pigeon bit repeatedly by a viper in the leg, it being previously tied; in thirty minutes I took off the ligature; the leg was swelled and livid. In six hours its livid appearance was lessened, but
a swelling

a swelling appeared above the ligature. In twenty-four hours the leg was still swelled, and had a bluish cast: the swelling above the ligature had reached the abdomen and breast. In forty hours the pigeon died, the parts above the ligature being much discoloured.

I had a pigeon's leg bit repeatedly by a viper, and tied immediately after. I then had it bit several times by a second viper, and in an hour took off the ligature. In twenty-four hours the leg was swelled, but inconsiderably; in forty the pigeon was perfectly well.

I had a pigeon's leg bit twice by a viper, and instantly tied with a ribbon in the usual way. In four hours I took off the ligature, and found the leg exceedingly swelled and livid. In twenty-four hours these symptoms bordered upon sphacelus, and in thirty-six the pigeon died, with symptoms of disease above the ligature.

I had a pigeon's leg bit repeatedly by a viper, and immediately tied it. In twenty minutes I took off the ligature, and found the leg in a livid state, but scarcely swelled. In eight hours it was violently swelled, and livid. In twenty-four hours all was nearly in a gangrened state, and in thirty-nine the pigeon died.

I had a pigeon's leg bit three times by a viper, and immediately made a ligature, which in thirty minutes I took off; the leg was swelled and discoloured, but in eight hours the swelling was diminished.

nished. In twenty-four it had almost regained its natural state, and in fifty was perfectly sound.

I had a pigeon's leg bit repeatedly by a viper, and immediately passed a ligature round it, which in forty two minutes, I removed, and found the leg livid and swelled. In eight hours these symptoms were much diminished, but a swelling and discoloration appeared above the ligature. In twenty-four hours every symptom of complaint was lessened, and in thirty-six scarcely perceptible. In sixty hours the pigeon was fully recovered.

I had a pigeon bit once in the leg by a viper, and instantly made a ligature, which in two hours I took off. The leg was much swelled and discoloured, but in twenty-four hours had almost resumed its natural colour, except at the places where the teeth entered, and which were covered with small dark black spots. In sixty hours all disappeared, and the pigeon recovered in three days.

I had a pigeon bit repeatedly in the leg by a viper, and immediately passed a ligature round it, which in an hour and an half I took off. The leg was at first livid and swelled, but in eight hours these symptoms were abated, and very considerably so in twenty-four. In thirty-six hours the part was scarcely discoloured, and in three days the pigeon was perfectly well.

I had a pigeon's leg bit repeatedly by a viper, and passed a loose ligature round it, which when done, I had bit several times by a second viper.

In

In thirty minutes I took off the bandage. The leg was swelled, and discoloured in the parts pierced by the teeth. In twenty-four hours these symptoms were abated, and the pigeon recovered on the third day.

I had another pigeon's leg bit several times by a viper, and after passing a weak ligature round it, had it bit by a second. In an hour I removed the ligature; the leg was livid and swelled; but these symptoms almost disappeared in the space of twenty-four hours. In forty-two the natural colour was well restored; the pigeon recovered in three days.

It seems that from all these cases this deduction may be made, that the ligature applied immediately, and left for a determined time on the part bitten, is a certain remedy against the venom of the viper. It entirely prevents the internal complaint, and we observe the animal to recover, although the external and local one continues to exist.

I have observed in general, that the local malady is violent in proportion to the tightness of the ligature, and the time it remains on the part. It is at least what several experiments, which, for brevity's sake I omit here, have indicated to me. It is of the greatest importance to know with some precision, the least possible time the ligature should be left on, and the least possible degree of force required to prevent the venom's communicating itself to the animal, without bringing on a gangrene of the parts.

As

As to the preffure of the ligature, it was fo very flight, that I could not have thought it capable of preventing the progrefs of the venom. I generally made ufe of a very thin foft ribbon, of four lines breadth at moft, which I paffed feveral times round the thigh, above the articulation of the tibia with the femur, and tied it with a knot. Sometimes I forbore the knot, tying the ribbon with a piece of fine thread. In this way I had a great number of pigeons bit in the leg, and removed the ligature an hour after; they all recovered, and none of them have fince died.

This experiment constantly fucceeds, when well made, and when the ligature is paffed round the thigh as it ought to be.

Now, if we confider that a very fmall quantity of venom kills a pigeon, and that in a few minutes, it will feem clear, that the ligature ought to be a ftill more certain remedy in animals larger than pigeons, and which die with far greater difficulty.

I was fo fully perfuaded of the efficacy of this method againft the bite of the viper, that I did not hesitate to addrefs a detail of it, enclosed, in a letter, to the Marquis de Condorcet, fecretary to the Royal Academy of fcience, begging that he would be pleafed to deposite it in the Academy. I informed him in this letter that my method was fo very certain, that of 100 pigeons bit in the leg by vipers, I fcarcely rifked the lofing one, although it is fo delicate a creature that the

smallest quantity of venom is sufficient to kill it. It will be seen by and by, what led me into an error as to the *generality* of this remedy, and how much we ought to be guarded against the most promising analogies, as I have already hinted several times in this work.

C H A P. II.

If the Bite of the Viper is naturally mortal to Man?

TILL now I have treated of the bite of the viper in animals; it is my present business to speak of it as it relates to man himself; and this forms the most useful part of the work. And here I am not afraid to advance freely, that the bite of the viper is not absolutely mortal to man, and that those have been mistaken who have regarded the disease caused by the viper's venom as one of the most dangerous, and from which it is impossible to recover.

We have seen that the lesser animals, such as small birds, die in a few minutes, from the venom of the viper, if they have but been bit effectually, or rather, if the viper has penetrated with its teeth

teeth far enough into the body, to leave there the quantity of venom it usually forces from them when it bites : this circumstance I have verified in more than 200 small birds. The smaller pigeons themselves are in the same predicament ; not one of them escapes death, if the viper is in good order, and they have been effectually bit : we must except here the small number of cases in which the venom introduced into the animal, is thrown out again with the blood, as the animal in these cases survives from not having had the complaint. Pigeons when bit, live longer than small birds, and the period of their existence after being bit, has an agreement with their size and weight, compared with those of the small birds themselves.

If from pigeons we pass to fowls that have been bit, we already find a very great difference, both as to the intenseness of the complaint, and the time they live. We have seen that many of them do not die, although several times bit, and that others die much later than the pigeons and small birds.

If a great number of experiments made on fowls be sufficient to establish an inductive proof, I do not hesitate to assert that the time of their death has likewise some relation to the size of their bodies.

What I have said of small birds and pigeons, in their relation to fowls, may be equally said of small guineapigs and rabbits, compared with the greater animals of their species. These small animals infallibly died when bit effectually, but a great number of the larger ones recovered, however they had been

bit. They had very dangerous illnesses proportioned to the number of vipers employed, and the number of bites they had received from them, but usually recovered.

I have observed that very small dogs die easily, even though bit by a single viper, and but once; but amongst the middle sized dogs, a vast many not only survive, but resist the poison perfectly well, without any remedy being administered. In proportion as the dogs become larger, they the better resist this poison; five bites from three vipers were not sufficient to kill a dog that weighed nearly sixty pounds.

Now let it be noticed that a man is about three times as big as the dog of which I speak, and let any one judge if a viper can kill him with a single bite!

It has perhaps never happened that a man has been bit by more than one viper, and should it ever happen, such cases will be always very rare, since it so seldom occurs that a viper bites a man more than once. A very few contrary cases do not create an obstacle. It does not appear then that the usual bite of a viper can be mortal to man; but there is an observation which I have verified in almost all the countries through which I have passed, and from which I could draw any information, that seems clearly to demonstrate that the venom of the viper is not naturally so. I observed that it is very rare to find two persons bit by the viper, above all in the country or on the mountains, who make
use

use of the same remedy. I have known some cured with the theriaca alone, taken internally or applied to the part ; others with common oil ; others with stimulants, such as the strongest spirituous liquors ; and others on the contrary with sedatives. In short, there is no kind of substance or medicine, that some one has not tried in this disease. What is very certain is, that with all this none have died ; at least I could never substantiate any case in which a grown person died with the simple bite of a viper. Now if we consider that persons bit by the viper recover, however they are treated, and even with remedies altogether contrary to each other, we shall soon see that the bite of the viper cannot be so dangerous as has hitherto been imagined. A complaint yielding to all these remedies, even to those of opposite natures, can never be a dangerous one.

I have informed myself very carefully, in all the countries through which I have occasionally passed, of persons bit by the viper ; I have myself examined more than ten or a dozen, and have heard mention of more than fifty, either from physicians, surgeons, or persons who were present and assisted the sick. Of so many bitten, not one is dead ; and I have never heard but of two persons who are said to have died from having neglected to take remedies ! It was not possible for me to gain any information of one of these people, whatever enquiries I made into the subject, so that I even doubt the truth of the fact ; but I know that the other died at the end of twenty days, with a gangrened arm. He was scarcely bit, when

very deep scarifications were made, and in three days the parts had already gangrened. The Count de Carburi, consulting physician to the King, at Paris, saw eight persons in the hospital at Turin, all of them bit by the viper, and saw them all recover, although each had a particular treatment. Of these eight, he treated one with the volatile alkali.

It remains now to reply to a difficulty, if it can however be called so.

Some one may perhaps object that my experiments were made on animals, and that the argument does not hold good, betwixt an animal and a man, betwixt a man and a dog. Difficulties of this kind have been made at all times, either through ignorance or envy, by those who are galled at the seeing others add to the number of new truths, or by those who are ignorant of the laws and correspondence which nature has established betwixt animals.

I should be ashamed to endeavour to prove that there is a perfect analogy in the cases I have adduced, and that we may very well draw conclusions betwixt an animal and a man: it is sufficient to peruse what Boerhaave, Mead, Albinus, and Morgagni, have written on the subject, and the use these great men have made of it.

Common oil was believed in England to be a certain remedy against the bite of the viper, and experiments were made with it on a man, in the presence of several members of the Royal Society of London.

The Royal Academy of Sciences of Paris having
been

been informed that an English peasant had found a peccifick in olive oil, and that he had made the experiment with it on himself, in the presence of several members of the Royal Society of London, the Academy deemed the discovery of such importance, that it deputed two of its members, Messieurs Geoffroi and Hunauld, to verify the experiment. These two Academicians had several pigeons bit, and likewise several chickens, two cats, a goose, a turkey cock, and eight dogs. The result of their experiments was that olive oil could not be regarded as a peccifick. No experiment was made upon the human species, and this illustrious body declared however that oil is in no way a peccifick against the bite of a viper, and that it has not the smallest efficacy in curing the disease caused by it (*a*).

The two Academicians made a few general observations on the animals bit by the viper, which are as follow.

I. That there is no coagulation in the blood, but on the contrary, every sign of fluidity.

II. That the serosity is extravasated into the cellular membrane, and has a bloody appearance.

III. That the arteries are empty and the veins filled.

IV. That the blood is coagulated in the auricles and ventricles of the heart, but without any consistence.

In the report Messrs. Geoffroi and Hunauld made on the subject, we likewise find the recital of two

(*a*) Mem. de l'Acad. Roy. des Sc. de Paris. Annee 1737.

persons cured, who had been bit by the viper ; but they were treated in a way more likely, in my opinion, to occasion their deaths, than to afford them relief.

To one was given a great quantity of Burgundy, and repeated scarifications made. This patient was actually ill for two months, whereas he would otherwife have probably recovered in two days, having only been bit in the finger.

The other was likewise bit in the finger : ligatures and scarifications were tried, and after all he was very ill. These two cases evidently demonstrate to me, that the bite of the viper is not much to be dreaded, since though so improperly treated, it did not occasion death.

Mead, a few years after, having read the experiments made by the Academy at Paris, on the inefficacy of oil in the bite of the viper, adopted them entirely without difficulty, and believed with this illustrious body, that oil could have no efficacy, and that they had decided well in the affair. After a judgment pronounced by so illustrious a body ; after Mead has subscribed to it ; I do not believe that any one will be sufficiently hardy to throw doubts on the application of my experiments, made on so many different kinds of animals, and repeated on so great a number of each species.

The venom of the viper is poisonous to all animals with warm blood ; at least I have not as yet been able to find any exception. Being in Italy, I extended my experiments to all the animals I was
able

able to procure, and the complaint discovered itself in each of them when the viper had actually deposited its venom in the parts bitten. In every species of animal the complaint is violent and deadly in proportion to the smallness of the creature bitten; and its violence also depends on the quantity of the venom introduced. However, to suppose that what deranges the animal economy with so much activity and strength, in different animals, and encreases its effects in proportion to the encrease of its bulk, can at the same time be not at all injurious to man, would be hazarding a supposition at once absurd, incredible, and unlikely. Let a single example be brought of a poisonous substance, whether animal, vegetable, or mineral, which kills, or is productive of violent complaints in so short a space, in animals with warm blood, and has with all this no baneful effects on man, and it may then be said that the analogy betwixt the effects of a poison on man, and of the same poison on animals, is not admissible; or rather it may be said that this is a singular case, an exception to the general rule. The examples of cats, which survive the bites of several vipers, instead of favouring, destroy this hypothesis. It is true that the cat, a fierce animal, makes a strong resistance to this poison; but it resists it precisely because it is in comparison with other animals, very strong and robust, and is nevertheless, in spite of its strength, itself attacked with the complaint caused by the bite of the viper; and this complaint becomes violent, painful, and lasting in proportion as the quantity

ty of the venom introduced into the body of the creature by the viper is increased. I doubt not, if instead of five or six vipers, ten or fifteen for example had been employed, but the cat would have died, because the disease would then have been very violent, and more than equal to the strength of the animal. In fact, kittens die as well as other creatures, although bit by very few vipers, precisely because they are less robust than when grown up.

Reply to Monsieur Jussieu.

It now remains to reply to a difficulty which favours the volatile alkali, and which, previous to my experiments, might have appeared very great and unanswerable. This difficulty solely consists in the cases where persons bit by the viper were happily cured by the volatile alkali; such for example as the fine cure we read of in the history of the Academy of Sciences of Paris, by M. Jussieu, of a young man who was bit by a viper, and treated with eau de luce.

I think I ought to enter upon my reply by remarking that there are very few cases so complete in their circumstances, as that of Monsieur Jussieu undoubtedly is. Sannini indeed speaks of three persons bit by snakes, and cured by the volatile alkali alone; but we are ignorant of the quality and effects of the venom of these snakes, which certainly were not vipers, although he believed them more noxious than
the

the rattlesnake itself. Besides, Mead is of opinion that the rattlesnake kills in a very short space of time, even in a few seconds. The first of the three persons Sannini treated with the volatile alkali, had been bit several hours before the remedy was applied, and was notwithstanding so well recovered on the following day, that he returned to his usual employment of fishing. This Author likewise speaks of a little insect called centipedes, the bite of which he deems mortal, and says that he was himself cured of it by the volatile alkali. But neither is the strength of the venom of this animal well known, nor have there been sufficient experiments on the subject.

Monfieur de Mascena speaks of a cure made with the volatile alkali, in French Guiana, on a person bit by a snake. He was treated with eau de luce, and recovered (*a*). The snake is not named, and it is not known whether or no it is really noxious; but could it even be proved that a person bit by the rattlesnake had recovered on using the volatile alkali, ought we thence to conclude that the volatile alkali is a specifick against the bite of that creature? I have already proved in a demonstrative way, that it is certainly not so against the bite of our viper, which does not differ essentially from the rattlesnake, except in its size. It is true that the latter being seven or eight times as large as our viper, is consequently capable of conveying seven or eight times the quantity of venom, from whence the dis-

(*a*) Journal de Phys. août, 1777.

case may become proportionably greater and more dangerous. Doctor Mead speaks of a man who was bit in London by a rattlesnake, and who recovered by having the part sucked, and by bringing on vomitings with oil and water. This case may lead one to suspect that the bite of the rattlesnake itself is not always mortal, since the bite of our viper is not cured to a certainty either by suction or by vomitings brought on by swallowing oil. But why should the bite of the rattlesnake be invariably mortal to so large a creature as man, when we have seen that five bites received from several vipers were not sufficient to kill a dog, weighing only a third of a man's weight? I cannot indeed see why the bite of a rattlesnake, which may be reckoned equal to that of seven or eight vipers, should always be fatal to the human species.

The greater quantity of the rattlesnake's venom is not then a conclusive argument that it must invariably kill so large a creature as man; and why should it be equally dangerous, whatever part of the body may be bit, at what time soever it may happen, and in whatever state it may be found?

It has been seen above that wounds or bites in the noses and ears of animals are attended with little danger; the same may be said of those of the skin, which heal better than the muscular ones.

But were it even granted that the rattle-snake may by its bite introduce a sufficient quantity of venom to kill a man, how many causes may there not be to prevent the introduction of this quantity of venom into the wound so as to produce such an effect?

In

In the course of our experiments on European vipers, we have seen many cases in which there has been little or no complaint in consequence of the venom introduced: and what is the poison that may not become innocent when diminished in quantity? The rattlesnake may be destitute of venom, a circumstance which I have sometimes observed in our vipers. It may bite so superficially, or so ill, that the poison introduced may not be sufficient to occasion death. A vein, an artery, torn by its tooth, is sometimes capable of forcing out either all the the poison, or a part of it at least. We have observed all these cases as they relate to our vipers, and they may be equally applicable to rattlesnakes.

To form a proper judgment of the strength of a rattlesnake's venom, and that of the other snakes of French Guiana, and likewise of the advantages of the volatile alkali in the disease caused by it, it would be necessary to make a great number of experiments, as I have hitherto done on the European vipers. Besides, if the volatile alkali is totally useless in the bite of our viper, how can it have any efficacy, how can it be a certain remedy in the bite of snakes which they pretend to be much more venomous? I am inclined to believe that the common bites of these snakes are not deadly in their nature, but that they become so in some particular cases and by accident, as by the number of bites received, and the injudicious manner of treating the part bitten.

If attention be paid to the complaint produced
by

by the viper's venom, it will soon appear that the animal bitten may easily die if badly treated, or if disorders are excited in its economy by persons little skilled in this disease, which is not unlikely. A large tumour is generally formed round the part bitten; there is an extravasation of black and livid blood in the cellular membrane, even at a great distance from the bite; and lastly, a considerable gangrene is frequently formed, which destroys the skin and cellular membrane, and extends to the muscles. Who does not see that in these cases the gangrene of itself, and uninfluenced by the venom, may be fatal, if the patient be by accident badly treated? And this may be the case in the instance of any one's dying from the bite of the viper: there remains a large local wound, which may be considered as caused by the mere mechanical action, and this wound may be well or badly treated. We have seen above that the venom of the viper kills by its general action on the animal, which does not die from the simple local malady of the part bitten, but is destroyed by the venom, although the part injured be wholly extirpated.

Although I have not had the good fortune to discover a certain specifick against the bite of the viper, I have however the pleasure to assure the publick, that this bite is not so dangerous as has been hitherto universally believed, and that in an instance where a person may have the misfortune to be bit, life should not be despaired of, even though no remedy be applied.

If

If I have demonstrated the inutility of a remedy which was believed a certain one, if I have destroyed the hope of discovering a specifick against the viper's venom, I have at least the consolation of having subdued the frightful idea that has been entertained, that the bite of the viper is usually mortal.

I am of opinion, that of 100 men bit, each of them once by a single viper, either in the foot or the hand, parts which are usually exposed to the bite of this animal, not one of them would probably die, even though they should make use of no remedy.

After having seen the effects of the viper's bite on eight different kinds of animals, as well those with warm blood, as those with cold; after having had more than a thousand of them bit in so many different parts of the body, by several vipers, and that repeatedly, I believe no one will deem the opinion I have delivered a rash one; an opinion which even becomes the necessary consequence of what we have hitherto observed.

The ligature made use of against the bite of the viper in the pigeons, was the immediate consequence of these very experiments. After having discovered that the viper's poison did not attack the nerves, that its whole action was exercised on the blood, and that the disease was only communicated by means of the circulation, it was easy to perceive that this circulation being stopped, the disease caused by the venom would be so too. I
tried

tried this method with the greatest success, and found it a certain remedy in the cases of the animals for which I made use of it. It did not however appear possible to me but that the ligature had been proposed by some author, since it is an idea that seems to present itself so readily. It is very true that Redi, who treats of the venom of the viper in two distinct works, makes no mention of the ligature, and Mead himself, who speaks of a great number of remedies, some even that are without efficacy, takes no notice of it.

I have for several years made researches into the remedies used in different countries against the bite of the viper, and could produce more than fifty recipes given me for the most part by country people, or by idlers. In one of these recipes mention is made of the ligature, but this ligature is joined to so many other things to be done before and afterwards, and all of them so absurd, that no one would ever take the pains to examine, if any thing were to be expected from the use of this method. In general these recipes not only contradict each other, but what is still more, one remedy is opposed to another in the same prescription. Some are introduced, having a sedative tendency, and others on the contrary which dispose to irritation. Some amongst them are cooling, others heating. The remedies themselves are in a great part absurd and ridiculous, notwithstanding which the person who gave me these recipes assured me of their efficacy, and several of them had been bit by
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vipers, and treated with them. I confess that I have not had the patience to verify any of them by experiment, except some few that seemed the least absurd, and these I found totally useless, and some of them even hurtful. But at length I found in an authour, a method of treating the disease caused by venomous snakes, in which the ligature has a considerable share. This authour is the celebrated Kempfer, who says he made use of it with the greatest success; in his voyages to India, and cured several persons by this method.

Had I known Kempfer's method before I made my experiments on the pigeons bit by the viper, and which were cured by the simple ligature, I should never have made use of it, and could not have believed it a certain remedy. I was too much persuaded that the nerves had a great share in the disease of the venom: it was necessary for me to know that all was brought about by the circulation; it was moreover necessary that the internal disease should not be communicated to the animal so as to occasion its death, till at the end of a certain time. I was then ignorant of all this, and was even persuaded to the contrary. The bite of the viper in pigeons, convinced me that the internal malady was already communicated to the animal in twenty seconds, and that its death was not occasioned by the external and local malady, but by the internal one. In these circumstances it was easy to believe that Kempfer's method would come too late, and would be useless on that very account

alone. But I should have had other reasons for not trying it : Kempfer, in all his voyages, makes no mention of vipers, but speaks very much of venomous snakes which are not yet well known. It is not yet determined whether the venom of these is analogous to that of the European viper, and whether it causes a like disease.

I could not besides have any confidence in a method which I should rather have thought a hurtful than a good one. That of Kempfer consists in several things which he describes as equally necessary against the bite of a snake. He begins by a ligature above the part bitten, and then proceeds to scarifications. He squeezes the blood from the wounded part, covers it amply with theriaca, and applies above all pieces of lincn spread with the same. During the whole course of the disease he gives his patient sudorificks.

Kempfer's method consists then, as we see, in five or six particular remedies, and it cannot be determined what good or what harm each of them may be productive of. Besides, every thing concurs to give me a suspicion of this method : there remains a doubt whether the ligature is proper or improper ; and I know by experience, that scarifications in the part are more hurtful than salutary, and that sudorificks, like the volatile alkali, are of no efficacy.

How many experiments ought not Kempfer to have made, to be certain that his method was good and effectual against the bites of the many snakes

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he speaks of, since to exclude to a certainty the fluid volatile alkali from the remedies employed in the bite of the European viper, I was obliged to make more than 600 experiments! he ought to have assured himself whether the venom of these snakes was in its nature mortal; he ought to have known the average number of animals destroyed by it; and he ought to have multiplied and varied his experiments a thousand ways on all the different kinds of these snakes.

But what is still more, Kempfer himself would have made me doubt his remedy, and his authority for it: in the same work in which he speaks of this remedy, he asserts that *the stone of Pedro de Cobra* cures equally well the bite of these snakes.

It is in the first place difficult to conceive why Kempfer has preferred the use of a long, complicated, painful, and difficult method, whilst he had one so easy, commodious, and certain as this stone, which is found every where in those countries: it must be confessed that this is far from giving any confidence. It is besides known from the experiments of those two great Italian naturalists, Redi and Valisnieri, that this stone has no efficacy in curing the bites of our vipers; whence it follows, either that the venom of these snakes Kempfer speaks of is totally different from that of our vipers, or that Kempfer asserts the truth of particulars absolutely false and badly attended to, and that his authority can on that account have no weight.

I now think myself at the close of my present work, and flatter myself that my labours may be of some utility.

The bite of the viper conveys to the persons bitten the dread of its being fatal, and terrifies whole families. The persuasion of the disease being mortal, and that not a moment is to be lost, causes the application of remedies either violent or hurtful. The dread itself may increase the complaint; and there have been persons who have received almost imperceptible bites in the hands or feet, and who perceiving an instant after a viper near them, have suddenly fainted away.

I knew a man who seeing himself bit by a viper, fell into a swoon from the simple fright; he remained in this state more than an hour, till he was observed by accident, and brought to himself by means of cold water thrown upon his face. Animals in general who seem the most to dread the bite of the viper, and who tremble at the very sight of this creature, die the soonest. Dogs, who are enraged when bit, and attack the vipers furiously, make a great resistance to this poison: I have at least thought I perceived so in the course of my experiments on these animals. It cannot be doubted but that violent affections of the mind, and the dread of approaching death, must strongly influence the state of a sickness in man.

A person may very well die under these circumstances, who would not have died from the complaint alone caused by the venom. A simple bite
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of a viper is not in its nature mortal; and were there even two or three vipers, although the complaint would become more violent, it would not be fatal in its effects. Should a man even be bit six or seven times by a viper, and should all the venom contained in its vesicles be conveyed into the wound, yet ought we not to despair of him. The complaint would become violent, but there is as yet no certainty that it would be mortal. A real consolation then, and a truly useful discovery, results from having better examined than heretofore, the effects of the viper's venom, on animals of different sizes, and on man himself.

Experiments on the utility of the Ligature against the bite of the Viper in small Birds.

Mere curiosity, and perhaps again the vanity of being able to cure with the simple ligature the smaller animals bit by the viper, induced me to undertake various experiments on sparrows that had been bit; and the result of these experiments created doubts as to the ligature itself in larger animals, which I should otherwise never have entertained. I had not even a momentary doubt, since a pigeon could be cured by the ligature, but that a rabbit, a dog, and even a man, might be cured with still greater ease. The analogy was not only applicable in this case, but the nature of the pigeon, the action of the venom on its blood, and

the effects it produces on the animal, were so many direct proofs that the ligature ought to be a remedy certain in proportion to the largeness of the animal, and the difficulty with which it dies. I was nevertheless deceived : so true it is that nature does not permit us to guess at her operations ; that we know very little beyond experiment ; and that we seem even to be forbid to reason on experiments themselves. But let us proceed to those on the sparrows.

I had a sparrow bit by a viper once in the leg, which had scarcely been bit when I bound it with a silk ribbon above the part. In thirty-five minutes I removed the ligature, and the sparrow died twenty minutes after.

I repeated this experiment in all its circumstances : the sparrow died ten minutes after the ligature was removed.

I had another sparrow bit once in the leg by a viper, and immediately applied a bandage above the bite : I took it off at the end of an hour, and half an hour after the sparrow died.

I repeated this experiment on another sparrow without varying the circumstances, except that I removed the ligature at the end of fifteen minutes ; in fifteen more the sparrow died.

I bound a sparrow's leg, and kept it in that state for four hours, when it was scarcely altered in its appearance. I then contrived that a viper should bite it repeatedly above the ligature, which after three hours I took off. At the end of twenty hours
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the sparrow seemed very lively, and fed. In eight days I found it dead, although the leg was entirely healed.

After all the cases hitherto related of the sparrows, it appears that the ligature may be sometimes an effectual remedy against the bite of the viper. The last sparrow, which died at the end of eight days, proves nothing against the ligature, since without it, it would have been dead in a few minutes. I observed besides, that the sparrows I kept caged, died of themselves, and that the smallest movements, or the least violence they suffered, in having them bit, or in applying a ligature, was sufficient to kill them. Amidst these doubts I thought it necessary to multiply the experiments, and to vary them still more.

I bound a sparrow's leg with a ribbon in the usual way, and had it bit by a viper. The bite was just above the ligature: in seven minutes the sparrow died.

I bound the leg of another sparrow as above, and had it twice bit by a viper. It died in five hours, although the ligature was not removed.

I tied the leg of another sparrow, and had it bit by a viper below the ligature. In eight hours it died, the ligature continuing on the leg.

Having tied the leg of another sparrow, I had it bit twice by a viper, and removed the ligature in four hours after. It died at the end of eight.

These new experiments shew that the ligature preserves the lives of sparrows bit by the viper,

but not always. The sparrows that die several hours after its being removed, certainly cannot appear to die of the internal disease, since the smallest quantity of the venom introduced into their blood is sufficient to kill them in a few minutes. It is likewise probable that some of them die because in having them bit by the viper, in binding their legs, and above all, in the removing the ligature, those who are thus employed, as well as those who hold them, must inevitably do them some injury. When the ligature is taken off, the leg is already livid and swelled all over. This can never be so well done but that the creature must clearly suffer from it. There are some of them who no longer support themselves, but beat their breasts and legs against the cage, and in this state can neither eat nor drink.

I cannot doubt, after having had a great number of them bit, and having wounded several others with venomous teeth, but that all these different causes concur, more or less, to render the ligature useless in sparrows. Some were tied before they were bit or wounded, others immediately after. In some the ligatures were removed at the end of four hours, in others sooner. Three I left constantly bound, and neither of them died, but I took care to nourish them, without doing them any injury. Their legs became black, and intirely dried up. At the end of twenty days they flew about the chamber, supporting themselves as well as they could with the extremities of their feet.

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Five others died in my hands almost at the moment I had taken off the ligature, and immediately after I had given them drink. Twelve others recovered perfectly, and the four last died in between six and ten hours. There were in all twenty-four.

However favourable the result of this last experiment was to the ligature, and however probable it appeared that several of these animals died from quite another cause than that of the venom, I was not yet altogether easy, and I thought it proper to repeat my experiments on animals of a larger kind, and of a different nature.

Ligatures made in the cases of Fowls bit by Vipers.

I had a fowl's leg bit several times by three vipers, and in three minutes bound it with a strong silk ribbon. In an hour I removed the ligature, and found the leg swelled and livid in all the parts beneath it. In three hours the fowl died. The whole substance of the muscles bit was diseased, and there were some marks of complaint even above the ligature, at the side of the belly, and in the breast.

I had a fowl's leg bit repeatedly by two vipers, and after four minutes tied it over the part bitten. In two hours the leg was very livid and much swelled. I took off the ligature, and at the end of twenty-two hours the leg still remained a little livid. The fowl recovered in two days.

I had another fowl bit in the leg repeatedly by two vipers, and after four minutes bound the limb
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which in an hour became swelled and livid. Two hours after I took off the ligature; in twenty-two the leg was less livid and swelled; and in four days the fowl recovered.

I bound a fowl's leg very tight, and had it bit repeatedly by two vipers. It could not support itself on this leg, which in two hours became swelled and livid. In eight hours I took off the ligature; after twenty-two the lividity still remained, and even appeared above the part that had been bound, in forty-seven the fowl died.

I had a fowl's leg repeatedly bit by two vipers, and at the end of a minute tied it. In three hours I took off the ligature, and found the leg swelled and very livid. Three hours after this the swelling and lividity had extended to above the ligature, and in six more the fowl died.

I had another fowl bit repeatedly by three vipers in the leg, which I tied two minutes after. In six hours I took off the ligature, and in six more the fowl died. The swelling had reached above the ligature.

I had the leg of another fowl bit repeatedly by three vipers, and tied it three minutes after. At the end of nine hours I took off the ligature; the leg was swelled and livid, and bled from every part of it. The fowl recovered by degrees, the leg having a yellow and green hue which continued several days.

I had the leg of a fowl bit by two vipers, and bound it immediately afterwards, but slightly; in
twelve

twelve hours I removed the ligature, three hours after which the fowl died. The swelling and livid colour of the leg had reached above the ligature.

I had another fowl bit repeatedly in the leg by two vipers, and immediately bound it, but tighter than in the preceeding experiments. I took off the ligature after twelve hours; in six more the fowl died, with the swelling and lividity extended to above the ligature.

I had the leg of another fowl bit repeatedly by two vipers, and tied it immediately, but still tighter than in the last experiment. In twelve hours I removed the ligature, and in two more the swelling and discoloration had extended above it. The fowl recovered in five days.

I had three fowls bit in the leg, each by two vipers, and bound the parts immediately. Within the space of six hours I removed the ligature from one of them, suffering it to remain on the other two for twenty-four. In two hours after removing it from the two last, one of them died;—the other recovered. The fowl whose ligature was removed in six hours, died in six more.

I had a fowl bit repeatedly by two vipers, in the leg, which I had previously well bound. It died at the end of twenty hours, although the ligature had not been removed.

These experiments made on fowls, raise great suspicions as to the efficacy of the ligature against the bite of the viper; it has even appeared to me, and I do not think myself deceived, that the local
malady

malady is greater with the ligature than without it. I observe this difference throughout the whole journal of my experiments, and it is difficult for me to have been mistaken, since I have regularly compared the local malady of the limbs tied, with that of those which were not so. Several fowls, as we have observed, die before the ligature is removed, and that at pretty regular intervals. After the experiments made on the pigeons which recovered with the ligature, all this appeared paradoxical to me, and I could not conceive it possible but that it ought to recover larger animals. I dreaded the not having made my experiments on the fowls as I ought to have done; that the ligatures were either too tight or too loose; or that I had removed them either too soon or too late. In a word every thing seemed to me more probable than that the ligature was either useless or hurtful.

In the midst of these doubts I determined to make my experiments on some other kinds of animals more extensive, and for this purpose made choice of rabbits and guineapigs.

Experiments on Guineapigs.

I bound the foot of a guineapig very tight, and had it bit repeatedly by two vipers. After twenty-four hours I removed the ligature, and found the foot swelled and livid. In thirty hours it was less
livid

livid, but more swelled : the creature recovered in four days.

I had the foot of a guineapig bit by a viper, and applied the ligature a few seconds after. In the space of an hour there were marks of disease in the part bitten. I removed the ligature, and after ten hours there were scarce any appearances of its having been bit.

I had the leg of a guineapig bit repeatedly by a viper, and a minute after I bound it. In fifteen minutes the foot was swelled and livid. I removed the ligature, and in ten hours the animal had scarce any marks of disease. It recovered in twenty-four.

I had the foot of a guineapig bit repeatedly by three vipers ; and bound it a minute after. In two minutes the symptoms of disease had already discovered themselves. At the end of twenty hours the foot was very much swelled and very livid. In twenty-four the leg discharged blood and *serum*. In two days there was some swelling above the ligature, and ten hours after, the part bitten was covered with an eschar. In six days a total recovery took place.

I had a guineapig bit repeatedly by a viper, in the foot, which in two minutes I tied. After twenty minutes I removed the ligature, and found already marks of disease in the leg. In an hour the tumour had reached the ligature. The creature died in twenty hours, with the leg swelled and livid ;
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and the lividity had extended to the muscles of the belly and breast.

I had the leg of a guineapig bit several times by a viper, and bound it at the end of two minutes: In twenty minutes I took off the ligature, and found the leg diseased in the usual way. In six hours it was still a little swelled, but not livid, and was healed in twenty-four.

I had a guineapig similar to the former one bit several times by two vipers in the leg, which I bound a minute after. In thirty minutes I removed the ligature. The animal had symptoms of disease in its foot, but recovered in less than three days.

I had a guineapig's foot bit repeatedly by a viper, and at the end of twenty seconds bound it. I removed the ligature fifteen minutes after, and already found marks of disease in the part. In two minutes more I perceived a motion of its head, as if it was convulsed, and in four hours it died. All the muscles of the leg, abdomen, and breast, were livid and inflamed.

I had another guineapig bit several times in the foot by a viper, and immediately after bound the part. In fifteen minutes I removed the ligature; the local malady discovering itself in the leg. In thirty-two hours there were scarcely any signs of complaint, and in the space of forty-two, a perfect recovery took place.

It appears by the experiments hitherto related; that the ligature is capable of curing guineapigs that have been bit in the foot by the viper.

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A comparative experiment was necessary to convince me that the bite of the viper was fatal to this species of animals. I had six guineapigs similar to those I have just spoken of, bit each of them in the leg by a single viper. They all died in less than twelve hours.

Although satisfied of the utility of the ligature, I thought however it would not be amiss to multiply my experiments still more, and to vary them in some of the particulars.

I had the foot of a guineapig bit several times by a viper, and bound it, but very loosely. In thirty minutes I removed the ligature, and found every appearance of disease in the part bitten : however in ten hours there was scarcely any swelling or lividity ; in thirty it was quite well.

I had another guineapig bit several times by a viper, in the foot, and bound it still more loosely than in the preceding experiment. In an hour I removed the ligature ; in ten there were scarcely any marks of disease, and in forty it recovered.

I had a guineapig bit repeatedly by a viper in the foot, and bound it, perhaps still less forcibly than in the last. In two hours I removed the ligature, and found the foot very livid and much swelled. In ten hours all was diminished, and in twenty-four there were scarcely any marks of disease.

I had the foot of a guineapig bit several times, and at the end of two minutes bound it. Soon as it was bound, I had it bit again repeatedly by a second viper. In thirty minutes I took off the ligature,

ture, which was very loose. In twenty-four hours the foot was swelled and very livid; in five days the animal recovered.

I had the foot of another guineapig repeatedly bit by a viper, and in ten minutes tied it. I then had it bit by a second one. In twenty minutes I removed the ligature, which was very loose. In twenty-four hours there were scarcely any signs of disease.

I had another guineapig bit in the foot repeatedly by a viper, and in three minutes bound it, causing it to be bit afresh by a second. In twenty-four hours there scarcely remained any appearance of disease in the foot.

The utility of the ligature seems to be more and more demonstrated, and it appears also that a very weak one is sufficient. It is true that it must be left on for some time, otherwise the internal malady is excited in the animal, and it dies in a short space after.

Various experiments made on guineapigs, bit as above by vipers, have pointed out to me that when the ligature is removed in ten minutes, or perhaps more, after they have been bit, the animal dies very suddenly, and that of the internal malady.

It is not difficult to discover when the guineapigs die of the internal malady. When the complaint begins to communicate itself internally, the guineapig moves its head in all directions, and seems to be convulsed. In this case death is certain, and follows in a short time. I made these experiments

on very small guineapigs, and I chose them so, that the consequences might be the less equivocal.

Experiments on Rabbits.

Not content with having tried the ligature on guineapigs, I wished to try it once more on rabbits. I generally employed small ones, beneath the middling size.

I had a rabbit's leg bit repeatedly by two vipers and tied it immediately after. In nine hours it was greatly swelled, with a discharge of blood. In this state I removed the ligature; twelve hours after which the leg was livid and gangrened, in thirty hours the rabbit died.

I had the leg of another rabbit bit repeatedly by two vipers, and tied it three minutes after. In an hour and an half I took off the ligature. In six hours the leg was very much swelled and livid about the part bitten. In thirty hours the leg was scarcely swelled, but remained livid. In three days the animal appeared to be recovered.

I had a third rabbit bit several times in the leg by two vipers, and in two minutes bound the part. After an hour I removed the ligature, and found the leg swelled. In the space of twenty-four a discharge ensued, and in three days the skin was corroded, and a sore formed. In six days the rabbit was well recovered.

I had the leg of a rabbit bit repeatedly by two vipers, and four minutes after I bound it. In an hour and an half I removed the ligature. In four hours the leg was very much swelled, and discharged a good deal. The rabbit died in thirty-six hours; the tumour of the leg having extended to above the ligature, where there was also a livid appearance.

I had three rabbits bit in the leg as above, but as they were destined for a comparison, made no use of the ligature. Two died in thirteen hours. The third had a sore in its leg, which was otherwise much diseased; but it survived.

The experiments as yet made on rabbits seem to prove that the ligature is not a certain remedy against the bite of the viper in these animals, since we have seen them die when it has been applied, and recover sometimes without it. I repeated these experiments on eight other rabbits, which I had bit in the leg, each by two vipers. The ligature was not removed till after six hours. Five of the eight died.

Seeing that the simple ligature was not useful to all animals, I wished to try if, joined to scarifications, it would be more so; and as the local disease is formed of blood, partly dissolved, partly coagulated, which corrodes and gangrenes the solids, I thought of uniting to the scarifications an antiseptick, such as the bark.

Ligatures and scarifications tried on Fowls and Rabbits.

I had a rabbit's leg bit by two vipers, and immediately tied it: in two hours the leg was swelled, livid, and bloody. In this state I made four longitudinal incisions into it, at the part where the vipers had bit, and absorbed with linen the blood that flowed from the incisions. In this experiment I found that the muscles were gangrened. The rabbit died in ten hours.

I had a fowl bit in the same way by two vipers, and immediately applied the ligature. In two hours the leg was swelled and livid: I made scarifications as above. In four days the leg was covered with an eschar, and in ten the fowl recovered.

I had another fowl bit by two vipers, in the leg, which I immediately tied. After two minutes I made the scarifications, washing the blood from the wounds for a long time with warm water, and covering the leg with linen. In two days a black eschar formed on the part; in three days and an half the fowl died.

I had the leg of another fowl bit by two vipers and immediately tied it, making the scarifications, and washing it with warm water. This being done, I strewed a great deal of powdered bark on the incisions, and covered the whole with linen. In twenty hours I removed the ligature, and the fowl recovered in a few days.

I repeated the above experiment in all its circumstances, on another fowl. After twenty hours I removed the ligature, and in twenty more the fowl died.

I had two chickens bit, each of them by two vipers, several times in the leg, which I bound a short time after. I made the scarifications, and bathed them lengthways with the volatile alkali, very much diluted with water. After eight hours I removed the ligature from one of them, which died in three hours more. At the same time I loosened the ligature of the other, and two days after it died.

I had a fowl bit in the leg by two vipers; I bound the part, scarified it, washed it, and covered it abundantly with the cortex. It died in the space of seven hours, even before the ligature was removed.

I had another fowl bit in the leg by two vipers, and immediately afterwards tied it. I made scarifications into it, and moistened it with warm water in which common salt had been dissolved. The fowl died in sixteen hours, the ligature still remaining on.

This experiment I repeated on two other fowls, and made use as above, of the dissolution of sea salt. In twenty-four hours I removed the ligatures, and in twenty-four hours more the fowls both died.

After making the scarifications, I tried the infusion of the bark on two other fowls bit as above.

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In twenty hours I removed the ligatures, and in twenty more the fowls both died.

I had another fowl bit by two vipers in the leg, which I immediately tied. I scarified it, washed it, and kept it immerfed in lime water which I had made warm, for twenty-five minutes. After twenty hours I removed the ligature. In three days the fowl died.

I made the same experiment on another fowl, keeping its leg for two hours in warm lime water. In twenty hours I took off the ligature, and in thirty-five the fowl died.

I repeated the ligature and scarifications on twelve other fowls, the leg of each having been bit by two vipers, and immediately tied. Four were scarified, and the parts kept during an hour in a strong infusion of bark in warm water. In four others they were kept for an hour in simple warm water, and those of the last four in a mixture of warm water and the volatile alkali. I covered the legs with linen, and in six hours removed the ligatures. Three of those treated with the bark, two treated with warm water, and three with the volatile alkali, died.

The result of the numerous latter experiments on the use of the ligature against the bite of the viper, neither affords us that certainty, nor that generality, we might have expected on beginning them. Not that the ligature ought to be rejected as totally useless, since we have found in a certain remedy to pigeons and guineapigs. It might become so to

other animals, and perhaps would be useful to all, were the circumstances with which it ought to be tried, better known. It appears in general, that nothing ought to be expected from scarifications, whether larger or smaller, or more or less simple, since we have seen the very animals sink under this operation, who would the easiest have recovered with ligatures only.

The ligature, by confining the blood to the part, produces a greater local complaint, and a stronger disposition to gangrene: for this reason it should be pretty loose, and ought to be removed as early as possible.

I cannot determine what utility it may be of in man, because I have made no direct experiments; but as I am of opinion that the viper's bite is not mortal to the human species, the ligature in this case can do no more than diminish the disease: a very slight one may perhaps be sufficient, and it may probably be removed in a little time. But experiments are wanting to enable me to speak to a certainty, and these experiments are very rarely to be made on men.

I wished to see if the disease which the viper's venom causes in animals, diminishes when the incisions are made above, below, or around the part bitten.

It seemed natural to suppose, that as the venom of the viper finds its way into the blood by the circulation, it would also introduce itself into the parts purposely wounded, at least into those that approached

proached the nearest to the part bitten. In this case it likewise seemed very probable, that the quantity of venom being thus diminished by its more extensive distribution, not only the internal disease, but also the external malady would be diminished too; and that by this means the gangrene of the part would be stopped, or at least rendered less dangerous.

But the following experiments shew the little dependence to be placed on analogical proofs, and probable reasonings in matters of fact.

I had the leg of a fowl bit repeatedly by a single, but very large viper; I made two small incisions into the inner part of the leg, above and below the part bitten. The fowl died at the end of an hour, with the part bitten considerably diseased, but without any change in the two artificial wounds.

I had another fowl bit repeatedly in the leg by a viper, and made a little incision into the muscles opposite to the part bitten, and a second one into the muscles of the other leg. In six hours the fowl was much diseased. In thirty hours the leg was livid, even at a great distance from the part. In sixty hours the fowl died, with the leg in a gangrened state. In all this time I observed no change in the two incisions.

I repeated this experiment on different animals with the same success, and never perceived that the artificial wounds were affected by the venom; so that it seems to be a truth established by experiment, that the venom once introduced into the blood, and

circulating with that fluid, may occasion death; but that it cannot communicate itself to simple incisions, even when made in the vicinity of the parts bitten.

I feel that I have been too prolix. I might have been less so, and perhaps even more clear, had I followed the synthetical, instead of the analytical method. I preferred the latter, presenting my experiments in the order in which I made them. I have not even dreaded the disclosing my errors, and the shewing how often I have been obliged to begin over again. The analytical method is certainly neither the shortest nor the most favourable to a writer; but it is the most certain, the most luminous, and the only one which leads immediately to a discovery. It inspires the reader with confidence, shows in what way the naturalist has searched into nature, and in what way she has answered to his researches. His faults are at the same time observed, the efforts he makes to come at the truth, and the difficulty of attaining it.

Works that present something new, ought all to be written in the way I have made choice of in proceeding thus far. When the methods are seen which have led to discovery, the merit of the work, and the author's opinions will be the best judged of; all will be free from that mystery and reserve which abound in writings formed on the synthetical plan, in which the traces are wanting that have guided to a discovery. But man loves rather to be admired than

than useful, marvellous than true, difficult than important.

I have made more than 6000 experiments; I have had more than 4000 animals bit; I have employed upwards of 3000 vipers, and may have been deceived; some essential circumstance may have escaped me: I may have neglected some other, not thinking it necessary; my consequences may have been too general, my experiments too few in number. In a word, I may very easily have been mistaken, and it would be almost impossible that I should never have been so in a matter so difficult, so obscure, and likewise so new. It is sufficient for me to declare the having written nothing but what I have seen, or at least have believed to see.

In reviewing my journal of experiments, I perceived some mistakes in it, and found that I had written in some places, what I could not possibly have observed. It likewise happened to me several times, in copying the experiments from the journal, to read in one way, and write in another. This is a new source of errors into which I may easily have fallen. How little certain are we, even in things we believe ourselves best skilled in, and in which we have the least apprehension of being mistaken! I know but one class of people who never err; those who do nothing, observe nothing, and make no experiments. All others are led into errors; and the more experiments they make, the more they are deceived: but we must not desist on that account from consulting nature, and ought not
to

to blush when a Newton himself, who was scarcely ever mistaken in the sublimest calculation, has been deceived in matters of mere fact and experiment.

It is incumbent on me likewise to observe, that a part of my experiments on the venom of the viper, was made in the rudest season, in winter. It is natural to conceive, that the vipers I employed could not be in full vigour; that they would exert less strength in biting the animals; and that not having been nourished for several months, their venom would be in a less quantity. I can easily conceive, that in a more favourable season, as in the summer, and in a warmer climate, the effects would be in some way different, and in general greater.

I might likewise have been deceived by those who furnished me with the vipers. I had a custom in the beginning, of returning those I had employed in biting the animals, when I had no occasion to kill them. I have every reason to believe, that the vipers already made use of, have been sold to me a second time: this I no sooner suspected, than I determined to kill them without reserve, after having employed them in my experiments.

On all these accounts, and perhaps on many others which I am ignorant of, my experiments might be liable to some variation, were they to be repeated; but this does not render the principal truths I have deduced from them less certain. I hope that my work will be distinguished by the experiments, inductions, observations, and consequences it contains. If my consequences are false,
and

and my inductions unjust, my readers will soon perceive it, and no error will be occasioned by them. But if I have been mistaken in the facts themselves, if I have not made my observations well, my mistakes will be communicated to others, and will serve as a basis to a false theory. It is for this reason that I have endeavoured to be as exact as possible in the facts, and have entered into a long detail on several of them. In several places I have described my experiments at length, and in a great number. I might have been less tedious; I might have given the simple results; but I must then have been believed on my word, and my readers have been deprived of the pleasure of judging for themselves, which can alone lead to evidence and conviction.

Besides the greater part of my experiments relate to questions entirely new, and in regard to which either nothing has been done before, or the observations badly made. It was therefore necessary to extend them in some measure, and I hope my readers will thank me for so doing. Now, that a basis of experiments, and of determined facts, is formed, on the bite of the viper, the naturalist will continue his researches with more ease, and will present them with more brevity.

A P P E N D I X

TO THE

R E S E A R C H E S

INTO THE

V E N O M O F T H E V I P E R.

TWO years after Having made at Paris, where I then was, the experiments on the viper's venom which have been related in this work, I was informed in London, on my making some stay there, that a certain specifick against the bite of this creature was just discovered in Italy. The little success I had met with in France and in Italy some years before, in my search into an efficacious remedy against the venom of this creature, gave me a very earnest desire to enrich my work with so important a discovery.

His

His Excellency, Count Belgioyoso, Ambassador from the Court of Vienna to London, who esteems the sciences because he knows the importance of them, had the civility not only to procure me the treatise published in Italy on this remedy, but likewise to give me one of the *stones* which are the subject of this treatise, and to which are ascribed the faculty of curing the bite of the viper. This had been sent from Milan, and had been prepared by the author himself. On this occasion I was shown several letters from Milan and Vienna, which related wonders of this new, but already famous remedy. Miracles had been done with it, they said, at the first of these two places, and they affirmed that the best physicians of that famous city had a knowledge of them. They likewise added, that the rare and very important discovery had been made, that the so much boasted *cobras stones* were nothing more than calcined hartshorn.

The treatise I read was entitled, *Treatise on the Efficacy of an Alexipharmick against the Venom of the Viper, by the Abbé de Tecmeyer (a)*. It contains divers experiments which deserve attention, and which tend to prove that calcined hartshorn is a certain remedy against this creature's bite.

The perusal of this treatise gave me a still greater desire of verifying, myself, the utility of this boasted remedy ; since the only way to be as-

(a) This treatise was printed in the *RACCOLTA DI OPUSCOLI SCELTI*, of Milan.

fured of an experimental truth, is to have recourse to experiment itself. The different cures related by the Abbé Tecmeyer, however brilliant and extraordinary they may be, are nevertheless neither so numerous nor so varied, as I at least should have desired them in so important a matter. Neither could I conceive that hartshorn, calcined only to blackness, as the Abbé Tecmeyer will have it, could be a certain remedy, whilst calcined white, in which way I had tried it in France, I had not found it of any value. I however thought it necessary before I ventured my opinion, to make a great number of experiments on different animals with this *stone*, which I shall agree with the authour in calling so.

It is likewise true that on revising this treatise, it appeared to me that the authour of this new specifick gave too much extension to his remedy, and that many things are there advanced too readily, which have either not been sufficiently proved, or are not altogether certain.

For example, he is of opinion that the small piece of burnt hartshorn applied externally to the wound made by the viper, heals it by virtue of the volatile alkaline salts contained in the hartshorn from which it is prepared.

He maintains that the viper's venom is principally composed of acid salts, and quotes the authority of Mead, and his own observations made with a microscope. He even says, that with this
venom

venom he has changed the blue dye of the turnesol red.

He believes that burnt hartshorn absorbs the venom of the viper, because it turns milk yellow, after it is removed from the part bitten.

He finds his remedy efficacious against this venom, ten hours, and even more after the animal has been bit by the viper, and when the swelling is very great, the symptoms very violent, and the indications of approaching death very certain.

He finds it equally useful against the bite of a mad-dog; and such is his good opinion of these marvellous *stones*, that he believes by virtue of them, the having cured wounds made in the posteriors of a man with the teeth and claws of a tiger.

Lastly, he does not think it impossible but that the tooth of the caiman, a species of crocodile, simply carried in the pocket, will cure the bite of a viper.

He asserts afterwards, that Redi is mistaken in supposing that the *cobras stones* are no specifick against the bite of the viper; and he believes that this celebrated physician made his experiments on false ones. The thing is certainly possible; but, if the true and efficacious stones are nothing more than pieces of badly calcined hartshorn, I do not see what could have induced them to impose on Redi, in giving him false for real ones, since the latter might so very easily have been made. Besides, it appears that Kempfer thought but little of these true stones, called by the Indians *de cobras di cabello*,
and

and that he did not place the smallest confidence in them. In his *Amœnitates Exoticæ* he speaks of them in the following manner: *de efficacia hujus lapidis, et quæ in dies cum ipso distinguuntur in India experimentis multa dicenda, inquirenda, dubitanda venirent* (a). *Saltem fateor ingenue penès me valorem lapidis semper mansisse in suspensò, dùm quid erroris, et fallaciæ sublatere posset propriis experimentis non exploraverim* (b). And he knew them so well that he describes them, and neither believes them natural stones nor engendered in the brain of the snake; he even seems disposed to think them made of hartshorn: *substantiam*, says he, in speaking of these stones, *obtinet firmam et duram, levem tamen, hic ibi porosam, et quodammodo corneam, ita ut appareat formatus ex cornu cervi in vapore vel liquore aliquo macerato tinctoque; nisi fortè fragmentum sit lapidis Connoor variegati ita hic lapis dictus à patria Connoor multatriæ provinciæ, lusitanis ibidem, Pedra frigide dicta à qualitate refrigerante, estquè triplicis differentiæ sive coloris, nimirum albus, citrinus et obscurè cæruleus, qui postremo nephritico lapidi in omnibus præter levitatem simillimus est. Quotquot videre mihi contingit per Indiam firmam et insularem prædictæ conditionis et figuræ fuerunt. Qualiscumque figuræ fuerint prima fronte apparebunt haud quaquàm naturales, et in cerebro viperæ; quod vulgo creditur, genitos esse, et ut frustra fuerit, qui illos in anguium capitibus quærerent* (c).

(a) *Kempheri Amœnitates Exoticæ. Lemgovix, 1712.*

Fasc. III. p. 579.

(b) *P. 580.——(c) P. 581.*

Such are the opinions of the Abbé Teemeyer in this treatise, and I confess they have appeared to me very singular.

But should it even be true that burnt hartshorn cures the bite of the viper, I can never be induced to believe that such an effect is due to the alkaline salts of this substance. I have demonstrated, so as to place it beyond a doubt, that the fluid alkali itself is of no utility in this disease, and that the venom of the viper in substance, mixed with alkaline salts, preserves all its activity, and kills as heretofore.

It is besides an error, that the venom of the viper is composed of salts, and that these salts are acid; and it is also false that it changes the tincture of violets red. In the course of my work I have already pointed out the error of Mead, and of other naturalists, who have succeeded him, as to the salts in the venom of the viper. It is singular to see mistakes repeated by others, which were refuted more than ten years ago.

The slight change of colour observed in the milk, which is in itself in a degree yellow, certainly cannot be caused by the hartshorn's absorbing the venom, on being applied to the part bitten; for a quantity of milk scarcely sufficient to cover the bit of hartshorn, will not become yellow if the venom of several vipers be united with it. This colour of the milk is caused by the blood's being absorbed by the hartshorn when applied to the

bitten part; and in fact it tinges it equally when applied to a part wounded but not envenomed.

But it is time to proceed to experiments, which can alone determine whether a piece of burnt hartshorn is, or is not, a certain remedy against the venom of the viper. It has been seen in several parts of this work, how little trust should be reposed in experiments, even when they appear the most constant. I have sometimes seen five, six, and even more animals recover, one after another of the bite of the viper, and shortly after as many of them die, without my having done any thing to them in either instance: and I have sometimes met with the same consequences in applying the same remedies to the same animals in the same circumstances. In one instance I should have judged that such a substance was a specifick against the bite of the viper, and in another that it was either hurtful or totally useless. This is the risk we incur in not sufficiently multiplying experiments. I do not pretend to have myself guarded against this inconvenience in all the parts of the present work on the venom of the viper, although it is true that I have varied and multiplied the experiments exceedingly, as much at least as my circumstances at the moment permitted me.

But in the present case I imagined that a limited number of experiments would suffice to determine the utility of this remedy. The many observations I had already made on the bite of the viper, and the knowledge they had given me of the animals

I wished

I wished to employ, enabled me to forbear the multiplying them still more.

The first thing to which I applied myself to succeed in my experiments, was to procure a sufficient number of pieces of hartshorn, prepared in the way described in the above cited treatise, and quite black. I had taken them from that part of the horn which is fixed in the animal's head. Applied to the tongue, they adhered strongly to it. I prepared many of them, and of this number I chose the best, to the end that my experiments might be made at the same time, on the same animals, and with the same circumstances. After having made use of them, I put them in milk or in wine, as the authour directs, and having left them there for several hours, I afterwards exposed them a great while to the sun, or to a gentle fire, till they adhered to the tongue as before. I had likewise the advantage, as I have mentioned before, of having had one of them brought from Italy. I made use of these *stones* several times, with the success that will be seen.

Before I give the principal consequences of my experiments, I think it proper to observe, that I began them in London in the month of March, and did not finish them till the latter end of the month of May. Although it was the mildest season that had been felt for some years in England, it did not prevent several days being very cold, in consequence of which my vipers seemed to me to be much benumbed and very inactive. In general I

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thought

thought that I found them less active there than in France, and less so in France than in Italy; so that the consequences of my experiments on these animals must differ sensibly, but only from greater to less. It is however true that vipers are venomous in all countries, and that their venom can kill. To accomplish this aim in cold climates, with as much certainty as in warm ones, it will be sufficient to make them bite the smallest animals, or to employ several to bite a single one. In this way the effects may be rendered nearly equal in all countries, and at all seasons. Thus then, the action of the viper's venom is in proportion to its quantity, when all other circumstances are absolutely equal; they however vary to such a degree, that one can scarcely pronounce any thing certain on them, even when every possible step has been taken to succeed, and to cause all the trials to be quite alike in their circumstances. Let us proceed to experiments.

I had a pigeon bit once in the right leg by a viper, and at the same instant applied to it the Italian *stone*, which immediately fastened, and remained on. Seven minutes after the pigeon gave tokens of disease, and was dead in twelve. I detached the stone by force, and put it in milk for other experiments.

To make a comparative one, I had another pigeon bit in the leg by a viper, and it died in sixteen minutes.

I expressed the venom from the teeth of a viper forced into the muscles of a pigeon's leg, and applied

plied the Italian *stone*, which fastened immediately to the wounds. The pigeon died in eighteen minutes, without the *stone* having detached itself.

I made the same experiment on another pigeon, with teeth taken from a second viper, and it died in twenty-two minutes.

I had a pigeon's leg bit once by a viper, and immediately applied the Italian *stone*, which did not loosen of itself. The pigeon died in four hours.

To make a comparative experiment, I had another pigeon bit once in the leg by a viper, and applied the *stone* wrapped in a piece of bladder, binding it to the part. The pigeon died in eight hours, the bandage having probably retarded the action of the venom.

Another pigeon bit in the leg by a viper, died in two hours, although the Italian *stone* still adhered to it.

I had another pigeon bit twice by a viper, and made a very small opening with a lancet at the place where the teeth had penetrated, immediately applying to it the Italian *stone*. The pigeon died in six minutes, the *stone* still adhering.

I had six other pigeons bit by as many vipers. To four I applied the *stone*, to the other two I did not. One of the last died twenty minutes after, the other in an hour. The first four all died in less than twenty minutes, and one of them at the end of eleven. The *stones* were still attached to the parts bitten.

This experiment was repeated on six other pigeons, to each of which I applied a *stone*. They all died; three in sixteen minutes, and three in twenty-seven. Five of the stones continued to adhere: the one which fell, belonged to a pigeon that was one of the latest to die.

Experiments on Quadrupeds.

Perfuated of the little efficacy of these *stones* in the cases of pigeons, I wished to see if they would be of more use to quadrupeds. I made choice of small guineapigs and very small rabbits.

I had a guineapig bit in the leg by a viper, and having dilated the wound a little, applied the Italian *stone* to it, which fastened very well. It died an hour after, the stone still remaining on.

I had the leg of a guineapig bit by a viper, as above. This one died before the stone was applied to it, and almost at the moment of its being bit: a very rare case, and such as I have observed only once in the course of my experiments on the venom of the viper.

I immediately had another bit in the same way, and applied nothing to it: it died four hours after.

At the close of these last experiments I had six guineapigs bit successively; to four I applied the *stone*, to the other two I did not. Three of the first died in two hours, and one of the two last in
twenty-

twenty-six minutes. The two others had no perceptible complaint.

These experiments on the guineapigs likewise bear witness to the inutility of the proposed remedy.

I however made still some others on rabbits, and can certify that the result of them was entirely conformable to that of the preceding ones. The detail of them here would be tiresome. The fact is, that they not only did not prove the *stone* useful against the bite of the viper, but gave the clearest evidence on the contrary, of its being totally inefficacious.

Let not particular cases be urged against me, either of animals recovered, or of men who have been bit and have not died, after the application of the *cobras stone*. Experiments on men prove nothing, since the viper's venom is not usually mortal to them, any more than it generally is to large animals. To determine if this *stone* is useful or not, experiments must be opposed to others made on animals on whom no remedy has been tried, and it is necessary to make a great number of them. For example, let one hundred animals, such as pigeons, small rabbits, and guineapigs, be collected, and let them be bit by as many vipers, an equal number of times in the same parts. Let half of these animals be treated with *cobras stones*, or other boasted remedies, and let those which remain be left to themselves. Let the number of those that die on each side be remarked, and if there is

a very sensible difference in favour of the remedy applied, I shall say that that remedy is probably useful. If the same experiment is repeated two or three times on the same number of animals, and the consequences are always the same as in the first case, I shall then say that the utility of the remedy is a truth demonstrated by experience, but it will not, on that account, be a specifick, a certain remedy. To be so, it will be necessary that none of the animals, or at least a very few of them, die. But after the many experiments I have made, I look upon this specifick as impossible, or at least I do not think it will ever be discovered. This is not a consoling idea, but it seems just. I do not wish to discourage any one, or to keep others from making new researches; but too sanguine a hope frequently causes that time to be lost, which might have been usefully employed.

I hope that certain persons will not be so easily disposed to believe in prodigies, and to trumpet forth dreams as very important discoveries; and that certain other persons will mistrust their own strength a little, and even sometimes their own experiments: for it is easier to believe than to judge, and it is likewise easier to see imperfectly than clearly.

Experiments according to the method proposed by Kempfer.

I shall finish my experiments on the venom of the viper, by a detail of what I have observed in
making

making trial of Kempfer's method against the bite of that animal, that is to say, in employing scarifications and the theriaca. I relate it the more willingly, since in trying Kempfer's method, I thought it proper to make some experiments which ought to be rendered publick.

Kempfer proposes theriaca, scarifications, and ligatures, as a certain remedy against the venom of the viper and other snakes. In the course of my experiments I have already proved the inutility of theriaca applied to the part bitten, or taken internally; and have observed that scarifications and ligatures, instead of being useful, are considerably injurious. It is true that I never have united these remedies; it would, however, appear to me very singular, could they be only useful when joined together. It is the more necessary to have recourse to experiment, since Kempfer, a very grave author, assures us that he has found his remedy constantly efficacious, and has cured all the persons to whom he could apply it in time.

I had a guineapig bit once by a viper, either in the leg or foot. Having applied a ligature, I made slight scarifications in the part, squeezed out the blood, and covered the whole with theriaca, obliging the animal to swallow the same, mixed with water. The guineapig lived, but a part of the foot gangrened, and it was never afterwards of any use.

I had another guineapig bit twice in the foot by a viper. Having made the ligature, I scarified the part slightly, pressed the blood from it, and covered

vered all with theriaca, with which I rubbed the foot well. The animal swallowed a great deal of theriaca mixed with water; it lost the use of its foot, but recovered.

By way of making comparative experiments, I prepared two guineapigs as above, but only made the ligature and scarifications, neither applying the theriaca, nor giving it internally. One died in five hours; the other lived, but like the former ones, lost its foot.

The consequences of these experiments are neither uniform nor in sufficient number, to decide as to the inutility of Kempfer's method. I thought it necessary to make fresh experiments, and to vary them a little, operating likewise on different animals.

I had the leg of a guineapig bit twice by a viper. It was tied and scarified, the blood squeezed from it, then well covered with theriaca, which, when dissolved, I made the animal drink repeatedly. It died in two hours.

Another guineapig, somewhat smaller, was treated in the same way, and died in four hours.

I had another guineapig bit as usual, and did no more than scarify and apply the ligature: it died in four hours and an half.

I had another, a much larger one, bit, and did not treat it at all. It died in three hours.

I had four others bit by as many vipers, each twice in the foot, and treated all of them according to Kempfer's method. Two died in less than
four

four hours; the other two came off each with the loss of a foot.

Having had six other guineapigs bit as above, I treated three as usual, but not the other three. Two of those treated died, and the third recovered without losing its foot. As to the others, one of them died, another was very ill, and the third recovered, but lost its foot.

With some ones I afterwards had bit, I tried scarifications and ligatures, and covered the wounds with theriaca, without making them swallow any; others, on the contrary, swallowed the theriaca, without having scarifications made, or ligatures or theriaca applied to the part bitten. The consequences appeared to me to indicate the inefficacy of the theriaca applied to the part, and to admit conclusions that the scarifications and ligatures do a great deal more harm than good, because in general they dispose the parts to gangrene the readier. I could not determine that the theriaca taken internally was useless, since the events, though neither sufficiently constant nor numerous, were more favourable to it than not: but to be better assured of this, a greater number of experiments are necessary, than that I was enabled to make; and although the utility of it should be shown, I believe that many other substances capable of accelerating the motion of the blood, would be equally useful.

I made many other experiments on pigeons and small rabbits, conforming myself to Kempfer's method, but did not find them more favourable to
his

his system, than those cited above; so that I do not hesitate to declare that this method is neither certain nor useful, and that on the contrary it must appear in the highest degree dangerous, especially to large animals.

But whatever may be the inefficacy of the remedy proposed by Kempfer, I however found it singular that several of the pigeons recovered, although the disease of the venom declared itself with the most violent symptoms. This appeared to me so strange, that I determined to repeat several experiments, and to examine afresh whether different substances I had tried before, and found totally useless against this poison, were really so.

Substances employed against the Bite of the Viper; to wit, quicklime, magnesia, caustick alkali, absorbent earths, and calcined hartshorn.

I had a pigeon bit in the leg by a viper, and having made two slight scarifications, I covered the part with quicklime, which I kept on with a loose bandage. The pigeon had the disease of the venom; the leg swelled and blackened, and a fore formed itself: but in six days all came about.

Having had another pigeon bit as above in the foot, and having scarified, I applied quicklime; it died in twenty minutes.

I re-

I repeated the same experiment on two other pigeons; they were both very ill, but neither died: in seven days they were perfectly recovered.

I was desirous of repeating the same experiment on six other pigeons; two only died, although all the six had the disease: however one of them lost its foot by a gangrene.

I took two of these recovered pigeons, and had them bit each several times in the leg, by two vipers. Having made the accustomed scarifications, I applied the quicklime; one died in twenty-seven minutes, the other in six hours.

Of six other pigeons bit and treated as above, with scarifications and quicklime, two died, the other four all recovered in nine days. Two of them had the muscles of their legs so gangrened, that they never could use them afterwards.

I repeated the same experiments on small guinea-pigs and rabbits, and the consequences of them were far less favourable to the use of the quicklime, than in the cases of the pigeons. I however in my first attempts fancied it not to be totally useless; but however it be with quadrupeds, it is certain that I deemed it of use to pigeons, all of which usually die when the disease of the venom communicates itself to the part bitten: such is at least the result of experiments made at Paris. The pigeons' legs when bit, became swelled and livid, with symptoms of gangrene; and several of the abdominal muscles, as well as all those surrounding the wound, were black.

It is besides true that I have had consequences very analogous to those of the lime, on applying absorbent earths, such as the different boles, but above all, English pipe-clay, to the parts bitten. Many of the pigeons recovered, although the greatest number died, with all the symptoms of the disease of the venom.

However I very much suspect the utility of these remedies, and the cures they have effected, because I have met with several recoveries without applying any remedy at all. I have had pigeons repeatedly bit, and the venom was well communicated to the parts, since several of them lost their feet by the gangrene which supervened; the inflammation and stoppage of circulation were extended to a great part of the body, and the animals recovered, although not perfectly till at the end of eighteen or twenty days. I generally observed at Paris, that the smallest quantity of venom was sufficient to kill a pigeon, since it had symptoms of the disease, so that I am now persuaded that there may very easily be a difference betwixt the venom of one viper, and that of another; betwixt the venom of vipers of one country, and of those of another; and betwixt the venom of the same vipers at different seasons. In this way may be conceived why great scorpions are mortal in summer, and not in winter; and why the pigeons bit by a single viper, and treated with oil before several of the Members of the Royal Society at London, recovered. It must, however, be observed, that it is not impossible but
that

that the poison introduced into the animal was not sufficient in quantity to produce a dangerous complaint. We have seen several similar cases in the course of the preceding experiments.

TREA.

T R E A T I S E

O N T H E

A M E R I C A N P O I S O N

C A L L E D

T I C U N A S*,

A N D O N S O M E O T H E R

V E G E T A B L E P O I S O N S.

THE experiments I made at Paris four years ago on the venom of the viper, and which are a continuation of many others I had published ten years ago in Italy on the same subject, have enabled me to speak with certainty on the nature and properties of this poison. The unexpected and important effects which I have observed in applying

* This poison is thus called from the name of the Indians, amongst whom it is prepared. Mem. de l'Acad. des Sciences. Ann. 1745, p. 490.

the venom of this animal to living bodies, have presented me new truths as to the animal economy; and these new truths have by degrees conducted me to a doubt of certain medical theories which are not sufficiently proved, or which have become too general amongst practitioners.

I have since wished to extend my researches to other venomous substances, and was desirous to examine, if possible, one of the most active vegetable poisons. I figured to myself that animal poisons, as for example, that of the viper, applied to a wound, do in truth diffuse themselves in the body of the animal, but do not on that account become augmented, as does on the contrary the *variolous* venom (*a*), or that of a mad animal. I figured to myself, I say, that these poisons had a great analogy betwixt them, and that they acted in the same way, and on the same parts of the animal: but on the other hand, I could conjecture nothing as to the action of vegetable poisons, which I had not yet examined, and it did not seem to me possible to establish any certain principles on them, even after reading the principal symptoms they excite. The mode of making experiments, which had been followed, was very different from that I had employed in examining the venom of the viper, and the deductions drawn from them seemed to be too vague and uncertain.

(*a*) The matter of the small-pox.

On my arrival in London, I was enabled to satisfy myself with ease on the subject. Mr. Herberden, a celebrated physician there, and member of the Royal Society, procured me a great number of American arrows well preserved, and well covered with the poison; he had besides the complaisance to get me a good quantity of the poison itself. I found it enclosed and sealed in an earthen jar, the tin case of which still remained on it. Within the case was found a paper, on which was written as follows: *Indian poison brought from the banks of the Amazons by Don Pedro Maldonado: it is one of the sorts mentioned in the Philosophical Transactions, vol. 47, No. 12.*

In this volume of the Philosophical Transactions mention is made of two poisons of pretty much the same activity; one called *Lama*, the other *Ticunas*.

The poison contained in the earthen jar, which I made use of, is the *Ticunas*. It is not well known with which the poisoned arrows were prepared; but I found by experiment, that it had the same strength as the *Ticunas*, so that I thought it needless to distinguish one from the other.

Many things have been written on the activity of the American poisons, so that I thought it advisable to begin my experiments gradually, taking all possible precautions. It is thought that the smell alone on opening the vessel containing it is hurtful, and grievous diseases, and even death, are apprehended from the escape of some of the particles of it.

it into the air; this at least is what we read of in the graveſt authours.

I began then, as ſoon as the jar of poiſon was opened, to make a young pigeon inhale the impregnated air, and kept it with its head in the jar for ſeveral minutes; when I drew it away, 'twas as well as before. I looſened with a chiſel ſeveral bits of poiſon, to raiſe a little duſt in the jar, and again plunged into it the pigeon's head; it did not ſuffer in this ſecond experiment, any more than in the firſt.

From this moment I did not hesitate to expoſe myſelf to this vapour, and to examine the ſmell of it, which ſeemed to me nauſeous and diſagreeable. Several very fine particles of it entered with the air into my mouth, and I found them to taſte pretty much like liquorice. Thus then is the ſmell of the dried poiſon innocent, and ſo likewise are the particles which find their way with the air into the mouth or noſe, and reach the lungs.

But it appears that the circumſtance in which they dread this poiſon the moſt, although it is ſtill external, is when it is reduced to vapours by live coals; or when it is boiled a long time, and riſes in a thick ſmoke. I caſt ſeveral ſmall pieces of the dried poiſon on burning coals, and made the pigeon inhale the ſmoke of them, in the miſt of which I kept its head: it did not betray the ſmalleſt ſymptom of uneaſineſs. I did more; I conveyed this ſmoke into a glaſs tube fix inches in height and four in diameter. When filled, I introduced the pigeon, and it

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did not seem to suffer more than if I had exposed it to the vapour of burnt sugar. I then sat about boiling a good quantity of it in an earthen vessel, and exposed the pigeon to the vapour that rose from it. I did this when the poison began to form a consistence; I exposed it when it was become more solid, beginning to burn at the sides of the vessel, and to reduce itself entirely into a very thick vapour, and into coal. The animal did not suffer in any of these trials, and I made no longer a difficulty of smelling to it, and of exposing myself to its vapour. The smell of the dried poison on the live coals is very disgusting, and resembles that of burnt excrements.

From all these experiments, I infer that the vapours which rise from the smoke of the American poison are innocent, whether smelled to, or inhaled; and Monsieur de la Condamine had certainly been imposed upon when he wrote that this poison is prepared by women condemned to death, and that they determine its having attained its point of perfection, when the vapours it emits, during the boiling, kill the person who attends it.

Not one of the well informed travellers who have visited the American continent, speaks of this tale they propagate of the accidents which happen to the old women destined to prepare the *Ticunas*. Monsieur de la Condamine himself only speaks of it after the doubtful relation of some native of that country; and on the like authorities he believes that salt and sugar are specifics against this poison. My experi-

periments have however taught me that they are of no use, and that it would be in vain to be flattered with the hope of obtaining a cure by these remedies, should any one have the misfortune to be really poisoned by the *Ticunas*.

There is no suspicion of the poison I employed in my experiments having suffered or lost its activity through age, so that we cannot ascribe to these causes, that the vapours which exhaled from it were not destructive, even to the most delicate animals. It had very well preserved its essential property of killing very strong ones, in a very short time, and in a very small dose ; and I was always unsuccessful in my endeavours to oppose *sugar* or *salt* to it, which are however Monsieur de la Condamine's two specifics, who has likewise in this adopted the opinion of the American natives.

This poison dissolves readily and perfectly in water, even cold ; as also in the mineral and vegetable acids. It dissolves much slower in oil of vitriol than in the other acids, and becomes black as ink, which does not happen in the other cases.

It neither effervesces with acids or alkalies, nor causes any kind of change in milk.

It neither turns the juice of radishes red nor green ; and when examined with a microscope, discovers nothing regular or saline, but seems chiefly composed of very small irregular spheroidal corpuscles, like those of the juices of vegetables. It dries without cracking, differing in that from the venom of

the viper, and when put on the tongue has a very bitter taste.

From all this I conclude that it is neither acid nor alkaline, and that it is not composed of salts visible to the microscope.

The order I meant to pursue in my experiments, rather than curiosity, engaged me to examine whether this poison would be mortal, if put in immediate contact with the eyes, or if it would bring on disease or irritation. I had already found that the venom of the viper is altogether innocent when in any way applied to the eyes, as it also is to the mouth and stomach; and was curious to see what agreements there might be betwixt two poisons so active, but so different in their origin. I began then by putting a small quantity of the *Ticunas* dissolved in water, on the eye of a guineapig: the animal neither seemed to suffer at the moment, nor afterwards, neither was the eye at all enflamed. I repeated this experiment two hours after on both eyes of the same animal, applying a greater quantity of the poison: its eyes retained their natural state, and it did not suffer the smallest inconvenience. I repeated this experiment on the eyes of two other guineapigs, with the same success; and such was likewise the result of all the experiments I afterwards made on the eyes of several other animals, and particularly on those of rabbits: I never could observe the smallest change in the part, to which I found that the poison was no more offensive, than if I had bathed it with
water :

water : whence I think I may conclude that the American poison is not a poison when applied to the eyes, and that it has no action on these parts.

But will it be as innocent when received into the stomach ?

Monsieur de la Condamine, and all the other authors who have spoken of this poison, believe it altogether innocent, taken internally, and such is the opinion of the Americans. The reason it is thought so, is that the animals killed with this poison, or rather with the poisoned arrows, can be eaten without danger. Such an argument is more specious than convincing, because this substance may be a poison when introduced into the blood, even in the smallest quantity, and may not be so when received into the mouth in a much greater quantity.

There is a relation in the British Journal, digested by Mr. Cleaby, (Vol. 13. p. 85) that a small bird which had been made to swallow this poison, died instantly. But this observation, isolated, and deprived of its necessary particulars, did not influence the minds of the authors who have treated of this poison, which continued to be notwithstanding regarded as entirely innocent when taken internally.

Here follow the experiments I have made on this subject : they still serve to make us cautious how we pronounce, even after having had recourse to experiment.

I made a small rabbit swallow two grains of poison dissolved in water, and forced it to drink a teaspoonful of water to wash its mouth, and carry

down all the poison into the stomach. This animal did not appear to suffer, neither at the time or afterwards.

I made another small rabbit drink, as above, three grains of the poison; it did not suffer any more than the former.

Four grains of the poison swallowed by another small rabbit, were attended with no ill consequences. I made the same trial on three small rabbits, to the third of which I gave six grains of the poison, and it continued well like all the others.

I thought these experiments might be sufficient to assure me that the American poison is innocent taken internally, as is the venom of the viper; but I was mistaken. I had the curiosity to try it on a young pigeon, which I made swallow six grains of it, and it died in less than twenty-five minutes. I repeated this experiment on two other pigeons, both of which died in thirty.

These last experiments, which seem to contradict the preceding ones, obliged me to make a great many new ones on rabbits and guineapigs. I made a small guineapig swallow five grains of the poison, and found it dead in twenty five minutes.—I made a small rabbit swallow even to eight grains of the poison; in thirty minutes it had no apparent complaint; in thirty more it began to totter on its feet; in four minutes more it fell insensible; and in four others was quite dead.—I made two rabbits and two guineapigs swallow each about ten grains of the poison; one of the rabbits died in less than forty-
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five minutes, and the two guineapigs in twenty minutes.

These consequences led me to imagine that a greater dose of the poison would cause a more certain death, and that the same quantity of it would produce different effects on the same animals, according to the state of the stomach. I had generally observed in making the above experiments, that animals, when they swallowed this poison on a full stomach, either were much longer in dying, or had no complaint. I made trial of this on three rabbits and two pigeons, which I kept a long time without nourishment: they all died in less than thirty-five minutes with only three grains of the poison. I repeated this experiment on five more of these animals, with their stomachs filled, and only one of them died.

I deduce from this as an established fact, that the American poison taken internally, is a poison; but that a considerable quantity of it is required to kill even a small animal.

The particulars related above of its being innocent in a small dose, and mortal in a greater one, induced me to believe that the viper's venom, which is innocent when taken internally in a small quantity, would be mortal if the dose of it were increased. The numbed sensation it causes on the tongue, and which continues so long, seems a sufficient proof that this venom is not altogether innocent in this way, and that taken in a greater quantity it may readily occasion death.

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I reserve to some other occasion the making this experiment, and shall then employ the venom of eighteen or twenty vipers, to be swallowed by a small animal on an empty stomach, and I venture to pronounce beforehand that it will probably die; since, if so very small a quantity of venom deprives the tongue of motion and feeling, that is to say, destroys the principles of animal life in this organ, a much greater quantity ought to destroy them in the organs more essential to life.

If we consider that the venom taken into the mouth must extend itself over a very great and almost moist surface, and mix itself with the aliments in the stomach, and that the inhalent vessels are very small, it will appear no longer surprising that it is not hurtful when taken in a small quantity, which is what we observe in the American poison.

I began my experiments on the latter, and employed a lancet, covered with a dissolution of it in water. With this instrument I wounded a small guineapig three times in the leg, at certain intervals; the lancet was well supplied with poison, but the animal did not suffer at all. I made the same trial on three other small guineapigs, and on a rabbit, and neither of them died or was disordered by it. In all these cases the blood issued freely from the wounds, whence I suspected that the poison, instead of communicating itself might have been forced out, as I had observed of the viper's venom, which was often prevented in that way from doing harm.

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My suspicion was soon confirmed by the experiments that followed. I impregnated a single thread with the poison, and passed it through the skin of a guineapig, near one of its teats : it had no complaint. I impregnated another thread three times doubled, and letting it first dry a little, as I feared that the poison was prevented from lying on the skin by the threads passing across, I passed it through the skin of a small rabbit's thigh, near its belly. In six minutes it appeared feeble, and began to tremble, and fell motionless a minute after ; from time to time it was seized with slight convulsions, and in six minutes more was dead.

I repeated the same experiment with the doubled thread, on two other rabbits and three guineapigs ; in six or seven minutes they became feeble, fell down, were seized with convulsions, and died in the space of thirty.

I wished to see if the American poison could be communicated to animals and be mortal to them, being simply applied to the skin scraped, or just entered with the point of a lancet. I had observed at Paris, that the venom of the viper did actually produce a local malady in these cases, and that it changed and corroded the skin, but did not go so far as to kill. The American poison, on the contrary, never produces the local malady, as I observed in making the experiments related above, and it leaves the wounded parts in their natural state, which essentially distinguishes it from the viper's venom.

I cut off the hair with scissors from the skin of
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a small guineapig's thigh, and scraped it slightly with a file. It did not bleed, but was moist, and covered with small red spots. I wet it with a drop of a solution of poison in water, and in ten minutes the creature was convulsed, and a short time after fell down motionless, being from time to time more or less convulsed : it died in twenty minutes, the skin to which the poison was applied not being changed. This experiment made on two other guineapigs, and a small rabbit, ended the same way ; they all died in less than twenty-seven minutes with very violent convulsions.

I wished to see if the larger animals could resist this poison, when simply applied to the punctured or scraped skin. I shaved the skin of a large rabbit, and punctured it slightly in several places with the point of a lancet, moistening it with several drops of the poison. In fifteen minutes the creature drooped, bowing its head at intervals, and being scarce able to support itself ; however in less than twenty minutes more it became as lively as ever. I repeated this experiment on another, but smaller rabbit ; in ten minutes it began to move to and fro' its head, and could scarcely walk or support itself on its feet ; but twenty minutes after it recovered its usual spirits.

I shaved about an inch of the skin of a very large rabbit, a little blood oozing from it. Upon this skin I put about three drops of a solution of the poison. In six minutes the rabbit was very feeble and much disordered, and a minute after fell as if dead,

dead, being scarcely observed to breathe. From time to time it was convulsed, but in less than forty-six minutes was able to walk very well; soon afterwards it began to eat, and appeared perfectly recovered.

I scraped the skin of a fowl's thigh, and applied the poison to it: no illness ensued, although I twice repeated the experiment on other parts of the skin.

I slightly scarified the skin of a pigeon's thigh, and applied to it the poison dissolved in water. In twenty-five minutes the pigeon became so weak as not to support itself, and at intervals was convulsed. Soon after it fell insensible, and remained in this state more than three hours. However it afterwards recovered gradually, so that in half an hour more it did not seem to have at all suffered.

This experiment was repeated on five other pigeons: three died in less than twenty minutes; the other two fell into convulsions, but afterwards recovered.

From experiments since made as well on birds as quadrupeds, I conclude that the American poison may be mortal, when applied to the skin slightly scratched; though not always, nor in all circumstances. The largest animals easiest resist the action of this poison, and even the weakest, when they do not die, soon become as well as before.

I wished to know what quantity of poison would kill an animal. I had made a like search in France in regard to the viper's venom, and had determined the quantity. I had great reason to presume that a

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very small portion of the American poison would be fatal to a small animal, since a drop or two of its solution, applied to the scraped skin, was capable of killing more than one ; but I wanted the precise quantity.

I moistened a very small bit of cotton with the fiftieth part of a drop of the solution of the poison, containing such a proportion of water, that the poison scarcely composed a fiftieth part. I introduced this into one of the muscles of a pigeon's leg ; the pigeon felt nothing from it.

Two hours after I introduced into another muscle an atom of the dried poison scarcely visible to the naked eye ; here again the pigeon did not suffer. I repeated this last experiment on three other pigeons, one of the bits of the dry poison being pretty large ; neither of them died, or appeared to suffer : however I found the bits of poison whole and undissolved by the humours of the wounded part.

I applied to the muscle of another pigeon a bit of cotton much larger than that above, and moistened it with about eight times the quantity of poison : in six minutes the pigeon fell, and died soon after. I applied bits of cotton moistened with the poison, pretty much in the manner of the foregoing, to two guineapigs ; one died in twelve minutes ; the other fell insensible in six, but soon after came to itself.

I conclude from these experiments, that it requires about the hundredth part of a grain of poison to kill a small animal, and that the poison must

necessarily dissolve, to occasion either death, or some sensible derangement of the animal economy.

I made several experiments to determine whether the American poison would be mortal or dangerous, when applied to wounds in the combs of fowls, or to the scratched ears of quadrupeds. The venom of the viper is not usually mortal in these parts, but the disease, which does not attack the envenomed comb, seizes the gills, which swell so violently as frequently to kill the animal.

I began by repeatedly wounding the combs of fowls, and applying to them the solution of the American poison; I did this twice by the medium of cotton wetted with it, without producing any complaint. But my experiments on the ears succeeded otherwise: after several fruitless attempts to communicate the poison by scraping or wounding the ears of rabbits, which did not appear to suffer, I at length succeeded in killing two in less than thirty minutes after my having applied a great deal of poison to the most fleshy part of the ear, which I had wounded in several places with the point of a lancet.

The experiments on the ears convinced me, that when there are but few blood-vessels, the complaint either does not communicate itself, or is not mortal. The American poison is very analogous in this to the venom of the viper: they are both entirely innocent, in whatever way they are applied to the tendons, above all if the latter are free from blood-vessels; and likewise to the other parts of the body
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that are so, such as the cellular membrane, or the ligaments : it would be needless to enter into a detail of these experiments, which would not only be tedious, but, as will be seen by what follows, unnecessary.

I wished to know whether the American poison was more destructive when insinuated into the muscles, than when applied to the skin with incisions made quite through it. A large guineapig which two days before had twice undergone the operation of poison applied to the incised skin, without any subsequent complaint, and a third time with very trifling symptoms, died in less than twelve minutes after the application of the poison to the divided fibres of one of the muscles of its leg. In three minutes it fell motionless, and with scarce any sign of life.

I repeated this experiment ten times, and all the animals, as well guineapigs as pigeons, and moderate sized rabbits, died ; so that I have no doubt but that the poisoned wounds of the muscles are more mortal than those of the skin and ears, and of the combs of fowls. The way to succeed the best is to take a pointed piece of wood of a spongy substance, well steeped in the poison, and to force it, when almost dry, into the substance of the bared muscle ; but this method did not succeed on my making trial of it on the combs of fowls ; I could observe no complaint, although the wood was well steeped, and left for several hours in the combs, pierced through and through.

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I now had recourse to the arrows, employing several in piercing the skins of animals, and many others in wounding the muscles. In the former cases several of the animals survived, the large rabbits making the strongest resistance; but in the latter not one escaped death.

I found in general, that the arrows are more deadly in their effects than the poison dissolved in water, and simply applied to the part wounded.

I observed that the poison of the arrows is more active and certain, if they are previously dipped in hot water. Their activity is still increased, if they are steeped in a solution of the poison boiled to a siropy consistence. Several pretty large animals, such as rabbits, tumbled motionless in less than two minutes, and before eight, were dead. Some of the smaller ones became ill in less than a minute.

I passed one of these arrows well dipped in the boiled poison, into the comb of a fowl, and left it there a whole day, without the animal's betraying any symptoms of pain. The day following I pierced the comb and gills of the same fowl through and through with two fresh arrows prepared as above, and left them there ten hours. The fowl still continued well in the second operation, and I afterwards passed an arrow into one of the muscles of its leg, when in forty-two minutes it died.

Are Acids and Alkalies capable of destroying the deadly quality of the TICUNAS?

Amongst the researches I proposed to myself in examining this poison, that of the changes it might undergo when united with acids or alkalies was one, as it had also been in examining the venom of the viper. I had found that neither the strongest mineral acids, nor the most active alkalies, deprived this venom of its hurtful qualities, and wished to see if they would act as little on the American poison. For this purpose I dissolved it in the three mineral acids, as also in distilled vinegar, and rum, and in a few hours made the following experiments.

I made slight incisions into the skin of a small guineapig, and wet it several times with the solution of the poison in nitrous acid. What the animal suffered seemed to result from the wounds and acid alone, and in an hour it became as lively as usual. Two hours after, I repeated this experiment on another part of the skin prepared in the same way, employing a solution of the poison in rum; in less than four minutes the animal died.

I wounded the skin of a small rabbit slightly, and applied to it several drops of a solution of the poison in oil of vitriol: the rabbit felt no ill effects from it. In four minutes I wounded another part of the skin as before, and applied to it several drops of a solution

tion of the poison in distilled vinegar: in four minutes more the animal fell, and died in six.

I prepared as usual the skin of a small rabbit, and wet it with a solution of the poison in marine acid: the animal did not suffer from it. Six hours after, I applied to another part of its skin a solution of poison in rum, and in forty-five minutes it fell into convulsions, but recovered in less than an hour.

From these first experiments it seems probable, that mineral acids render this poison quite innocent, and that vinegar and rum on the contrary, do not alter it. I continued my experiments on the solutions of it in vinegar and in rum, and the consequences were a little varied. Of six animals treated with the solution in vinegar, only two died; two others had all the symptoms of the disease caused by the poison; and the other two were not affected by it. Of six others treated with the solution in rum, five died, and the sixth had an attack of the disease; this seems to demonstrate that the *ticunas* dissolved in these two fluids, preserves its deadly qualities.

On the contrary, I repeated the experiments on the solution of the poison in mineral acids, on six animals, neither of which died, nor discovered symptoms of complaint.

I suspected that the poison might probably be innocent in this case, not because it had lost its deleterious qualities, but rather because the too great action of the mineral acids on the skin, and on the

vessels which are cauterized and hardened by them, might prevent its insinuating itself into the parts wounded. To clear up this doubt, I evaporated over the fire the solution in the mineral acids, and when the poison became dry, I applied it to different parts of the skin in several animals, but without any one being diseased by it.

It appears then, that the mineral acids deprive the American poison of its hurtful qualities. I say simply that it appears so, because it still may be suspected, that a little of the acid may remain in the poison after the evaporation, and that this acid may produce its usual effect on the vessels of the skin. I ought to have repeated these experiments after having washed the poison in several waters, but at that time I wanted animals to examine into the truth of this fresh suspicion, and have never since been able to return to the subject.

As to alkaline salts, I can venture to say, that I have not found them in any way to change this poison, or to render it less deadly than before. It is true that I neither repeated nor varied these experiments so much as necessary. I should have done this, if I had not found great difficulty in procuring animals, and had not had an eye to much more important experiments.

It was natural to suppose, since acids prevented the action of the poison on animals, that they might also be a remedy against it.

I got ready the skin of a small guineapig in the usual way, and covered it well with poison; in
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about forty seconds I washed it with the nitrous acid, and afterwards with pure water: the animal had no complaint. Two hours after I laid some of the poison on one of its muscles, and immediately applied the nitrous acid: it instantly fell convulsed and without strength, and in two minutes died.

I repeated this experiment on the muscles of another guineapig, and had scarcely applied the poison, when I washed the parts with nitrous acid a little diluted with water. Two minutes after it became convulsed, and died in four.

I poisoned the muscles of four pigeons as above, and washed them immediately after with nitrous acid. In a minute all the pigeons expired. Fearing this might be the effect of the nitrous acid, rather than of the poison, I tried nitrous acid very much weakened on four other pigeons, all of which died, though much later.

I wished to see if the simple application of nitrous acid to the muscles, would kill pigeons and small guineapigs. I made the experiment on two of each species. Both of the pigeons died soon after, but the guineapigs survived, although one of them suffered a good deal.

It seems then that acids are useless and dangerous, when applied to the poisoned muscles of an animal.

How long is the TICUNAS in discovering its deadly effects in Animals that have been poisoned with it?

I shall not speak of other remedies I have tried, because experience has convinced me that all are useless, whether applied soon or late, exteriorly or interiorly. When the poison is deeply fixed, when it is already introduced into the humours, every remedy comes too late.

A very nice enquiry, and one that might be useful in certain cases, remained to be made. My experiments on the viper's venom were the occasion of this enquiry into the American poison. I had determined the time the former requires to diffuse itself in the body of an animal, and in what time the cutting off the envenomed part may be useful, to prevent the venom from communicating itself by the circulation, to the animal.

I introduced an American arrow, previously dipped in hot water, into the muscles of a pigeon's leg, and left it there. In four minutes I made a pretty tight ligature about the part wounded, and just below the femur. In twenty-six hours the animal had no other complaint than that caused by the simple ligature, which on my taking out the arrow, I untied. The part was a little swelled and livid, and continued lame for several days, but did not occasion the animal's death.

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I pierced the mufcles of another pigeon with a fresh arrow, as above, and in fix minutes made the ligature, leaving the arrow in the part. In four minutes the pigeon could not fupport itfelf, and fhortly after fell infenfible, dying at the end of fix more.

I repeated this experiment on another pigeon, leaving the arrow in the mufcles. In eight minutes I bound the leg. Three minutes after, the creature became ill, but in a fhort fpace recovered. In twenty-fix hours it was ftill alive, although the mufcles were become livid. I took off the ligature, and it died two hours after.

I fubmitted a fourth pigeon to the fame trial, and made the ligature five minutes after, leaving the arrow in the mufcles. It died in two hours.

I repeated this experiment on four other pigeons, and in two minutes made the ligature : not one of them died. Ten hours after I took off the ligature, when three died, and the fourth recovered perfectly.

I made the fame experiment in all its circumftances on four other pigeons, except that I did not take off the ligature till thirty hours were elapfed. One only of them died, in two days. Its death was certainly owing to the tightnefs of the ligature, which produced a gangrene of the mufcles.

I repeated this experiment on much fmallier pigeons, that can bear the lofs of the leg beneath the femur ; not one of thofe died that had their

legs cut off in the space of two minutes, and only two out of ten, when they were taken off at the end of three.

Fewer pigeons die with this method than with the ligature, when the same time is observed: the reason is, that the amputation neither causes death, nor any remarkable derangement in the animals; instead of which the ligature frequently brings on a gangrene, which is sometimes mortal, in the parts wounded by the arrow.

I made the same experiments on small guinea-pigs and rabbits, sometimes cutting off the wounded leg, sometimes making the ligature: the consequences were partly analogous to those I noticed in the pigeons, though somewhat less constant and certain. I have in general observed that the American poison requires a certain time to communicate itself to the animal; that this time is much more considerable than that required by the venom of the viper; that the effects of the former on animals are more vague and uncertain; and, lastly, that the effects of both may be remedied by amputating the parts, when it can be attempted without danger, and is done in time.

In the experiments I made on the venom of the viper, I found that its effects are not alike to all animals, and that there are animals with cold blood to which it is quite innocent. I was curious to see if it would be the same with the American poison.

The authours who have spoken of this last, tell us that it is poisonous to all animals; but the belief

lief of a thing is very far from the proof of it. Experiments, and those very numerous, must be made, and I do not perceive that they have made a sufficient number from which a general consequence may be drawn.

Experiments on Animals with Cold Blood.

I began by insinuating the poison into the muscles of frogs, which died in a short space of time. I proceeded to eels, introducing the arrow near the tail; they all died though very late.

I had found that the venom of the viper is quite innocent to the viper itself, and to adders. I could procure only two of these last, and made but few experiments, which I however think decisive. I wounded one of these snakes towards its tail, with an arrow well covered with the poison, which had the consistence of a sirop, and left the weapon in the muscles. At the place where I insinuated it, I had previously made an incision, to the end that the dissolved poison on the arrow might the easier enter the muscles, into which I afterwards made small incisions about the wound, and introduced fresh poison. The adder continued well for several hours, when I shut it in a chamber, and on entering six hours after, found it fled, and have never since met with it.

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I repeated this experiment several times, at certain intervals, on a somewhat smaller adder. The last time, I forced two poisoned arrows into the muscles of its tail, and left them there for twenty-four hours. I applied the poison brought to aropy consistence, repeatedly about the wounds, introducing a great deal of it into them with a bit of wood; the adder neither died nor suffered perceptibly.

I was enabled to repeat this experiment several times on vipers, not one of which died of the poison, although I wounded several in the muscles near the tail with arrows well covered with it in the above consistence, leaving them in the muscles for twenty, and even thirty hours. It is true that some ones, a short time after the application of the poison seemed less lively than usual, and that the hinder part of the body which was wounded, became benumbed, and lost its natural motion in a sensible degree, and that for several hours. On the others the poison had no perceptible effect.

After this I can venture to say, that the American poison, as well as the venom of the viper, &c. is totally innocent to animals with cold blood. These two poisons have here a very great analogy, although one is an animal gum, the other a vegetable juice.

It remained for me to examine the action of this poison on living animals, and to see what parts of the animal are so changed by it, as to bring on death.

Every

Every thing concurred to the belief that it excites one of those diseases which modern physicians call nervous, and that it acts immediately on the nervous system. The symptoms of the disease are the most precise and decisive in favour of this species of complaints. Convulsions, faintings, total loss of strength and motion, the feeling weakened so as to be almost totally destroyed, are the most common ones produced by this poison, on animals. We often observe that the creature, just now very lively, in a moment finds itself deprived of motion and feeling, and at the point of death. I usually observed a symptom which effectually seems to demonstrate, that the disease produced by the *ticunas* is purely nervous. The animal, if it survived, was as well in a few minutes as before, not appearing to have suffered at all. It notwithstanding remained in a lethargick state, sometimes for several hours, without any apparent sign of life. This is exactly what happens in diseases called nervous, the attack of which is frequently very sudden. Now they excite commotions, and now entirely exhaust the strength; but scarcely do the symptoms of disease begin to dissipate, than the person finds himself recovered, and scarcely recollects the having been ill.

But all these symptoms could not impose upon me, after having made my experiments on the venom of the viper. The disease occasioned by it has likewise the symptoms common to nervous complaints, and the principal affection seems to lie
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in the nerves, although experience has decided to the contrary. It was likewise necessary then in the present case to have recourse to experiment, and not to be carried away by false theories and probable reasonings.

Effects of the TICUNAS on Blood drawn from Animals.

To proceed methodically in so important an enquiry, I thought it proper to begin by examining whether the American poison produced any sensible alteration in the blood of animals, if mixed with it on its issuing warm from the vessels.

I cut off a pigeon's head, and received the warm blood in two small conical glasses a little heated, about eighty drops in each glass. In one of them I put four drops of water, and in the other four drops of an aqueous solution of the *ticunas*, containing scarcely a grain of the dried poison. I shook each of the glasses for a few seconds, so as severally to unite their contents: in two minutes the blood mixed with the simple water was coagulated; that with the poison did not coagulate, but became darker coloured, and in three hours was still in a fluid state, whilst in the other glass the serum and coagulum were distinct.

I examined with a microscope, as well at this time as afterwards, the blood in the two glasses, and found that the red globules were alike in each, having preserved their primitive shape.

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This experiment was repeated several times with the same success, so that the American poison, in the circumstances related above, seems clearly not to change in a perceptible way the red globules of blood. It however deserves to be noticed, that far from coagulating this fluid, it on the contrary absolutely prevents the natural coagulation of it, when it is drawn from the vessels. Neither again can it be said to attenuate or dissolve it, since nothing of this is remarked on examining it with a microscope: the red part is shaped as usual, and does not seem to be more subtil or more fluid.

The consequences were the same on trying the viper's venom; so that the effects of these two poisons, or the changes they cause, in the blood drawn from the vessels, seem altogether similar. Both prevent it from coagulating, and neither of them dissolves or changes the globules of it: the only difference betwixt the two poisons is that the venom of the viper turns the blood darker than does the *ticunas*.

The former does not act on the globules of blood, even when it is communicated to the living animal, and kills it. I made the same observation as to the blood of animals which died of the American poison, so that in all these cases there is an admirable agreement betwixt the two. But it has been seen that the venom of the viper produces a sensible alteration on the general mass of blood in animals bitten. I thought it proper to examine with the
same

same attention, the blood of animals killed by the *ticunas*.

On doing this it appeared that the muscles of animals so killed, were in general rendered pale by it. The venous vessels near the heart seemed more swelled than usual, the blood a little darker, but not coagulated. The abdominal viscera were not sensibly changed; the heart and auricles in a natural state: the external vessels of the heart were sometimes more visible, and as if injected.

But I observed a great change in the lungs, a viscus very essential to life. I generally found it more or less spotted; the spots were frequently very large and livid, and sometimes the part seemed quite putrid. This change in so noble an organ deserves the utmost attention; it seems to become considerable in proportion to the length of time the animal lives after being poisoned. I found that the lungs of some animals were here and there transparent, above all towards the sides. The air within was very visible through the external membrane, which I examined with a microscope, and very clearly observed through it the small pulmonary vesicles moistened by a discharge from vessels that had quite emptied themselves.

However great the change in so important a part, I could not be persuaded that it could alone produce so violent and momentary a disease, and that the poison entirely exercised itself on the blood, and the lungs. 'Tis true that I had the example of the viper's venom acting somewhat in this way; but it
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brings on an almost general coagulation of the blood, which is certainly not the case with the American poison.

Effects of the TICUNAS introduced into the Blood Vessels of Animals.

In an enquiry so important, and at the same time so obscure, I thought it proper to recur to experiment, and to examine the effects of this poison introduced immediately into the blood.

I had recourse to the means I had employed in introducing into the jugular vein the venom of the viper, making use of a small glass syphon bent at the point, by way of syringe. Into this syphon I drew up by suction a solution of the *ticunas*, and having opened the vein, injected it. As I have already described the way of making these experiments, in the treatise on the venom of the viper, I do not think it necessary to repeat the description of them here. They are so contrived that the poison mixes with the blood through the medium of the jugular vein, without touching the part of the vessels where the incisions are made, or even the jugular itself.

I put into the syringe for the first experiment, four drops of an aqueous solution, which scarcely contained half a grain of the dried poison. Having introduced the point of the syringe into the jugular vein of a very large rabbit, I perceived the liquor

to flow back on the moment of my pushing the fucker, owing to its not being exactly fitted to the sides of the syringe: I observed to the persons present, that the experiment had failed, but was surprised to hear in reply, that the animal was dead. I do not think ten seconds passed betwixt the time of the liquor's flowing back, and the death of the animal, which had actually taken place. I cannot estimate the quantity of poison introduced into the blood, but as the animal died, some must necessarily have found its way thither; had not this happened, I should have supposed from the quantity which flowed back into the tube, that not a single drop of it had entered the jugular vein.

The death of this animal was much more sudden than in the cases of introducing in a similar way the venom of the viper into the blood; and the whole body was more sunk and relaxed, than it is observed to be in animals that have been dead a long time.

Having put my syringe in better order, I introduced two drops of water, with which I had previously mixed about a quarter of a drop of the aqueous solution of the poison I have spoken of. I scarcely began to inject this liquor by the jugular, when the rabbit fell, without motion and without life, as if struck by lightning. I do not think half a drop of the liquor was introduced.

I can say in general, after other experiments I have made since, that this poison introduced immediately into the blood by the jugular vein, kills
more

more suddenly than the viper's venom, and in a much smaller quantity. Death succeeds the introduction of it so quickly as usually to prevent convulsions. If a smaller proportion of it is employed, the convulsions and usual struggles are observed, and the death retarded. It is true that the blood is neither coagulated, nor so changed in its colour as when the viper's venom is mixed with it, but this does not delay the death of the animal, and the *ticunas* in immediate contact, kills to as great a certainty as the viper's venom. This is an experienced and an incontestible truth, however obscure and difficult the cause of the deaths may appear, in the cases I have related.

The American poison introduced into the blood kills instantly; from whence it seems beyond a doubt that when applied externally to the wound of a living animal, it must cause a derangement of the animal economy, and even death.

Effects of the TICUNAS on the Nerves.

The sudden death which follows the introduction of this poison by the jugular vein into the blood of an animal, seems an unanswerable demonstration, that in these cases the action of the poison is on the blood alone, and that the nervous system is neither affected nor deranged by it. This does not however prove but that the nerves may be more or less affected by this poison, when the death hap-

pens at some distance, and when it is applied externally to parts that have been wounded. In these cases convulsions, and all the symptoms of a nervous disease, are principally observed. The nerves then may very probably be affected by the poison, and be the principal cause of the animal's death.

Here it was again necessary to recur to direct experiment, as I had done in regard to the venom of the viper, and to see what derangements and what complaints the American poison would produce, on being applied immediately to the nerves, without touching the blood vessels.

Effects of the TICUNAS applied to the Surface of the Nerves.

I made my experiments on the sciatick nerves of very large rabbits, preparing the nerves in the way I had done at Paris in operating with the viper's venom, and shall therefore enter into no detail here as to that particular, but shall relate a few of the principal experiments, to show the variety I met with on my first trials: a variety that might have led me into an error, had I not persisted in multiplying my experiments, and varying them till I found the consequences somewhat uniform. 'Tis to this constancy, or if you will, to this obstinacy that I owe in a great part the new truths I think I have discovered, as well in relation to the viper's venom, as to the *ticunas*.

Having

Having cleared the sciatick nerve of a rabbit, I passed under it several doubles of fine linen, covering it with lint well wetted with the *tinctura* of a fiery consistence. I covered the nerve with the same linen, to secure the bared muscles of the animal from the poison, and sewed the skin in the usual way. In twenty minutes the rabbit began to be convulsed, and could not support itself; it was then seized with all the symptoms of poison, and died soon after.

I repeated this experiment on another rabbit, and contrived to enclose the nerve in linen still better than before. This second rabbit continued well for ten hours, but visiting it on the twelfth I found it just expired.

I suspected that the poison applied to the nerve in a certain quantity, might in time penetrate through the linen, with the humours of the wounded part, and convey its action to the muscles and other adjacent parts. It was proper then either to diminish the poison, to prevent its soaking through, or to cover it with more linen. I chose the latter as the surer method.

I cleared the sciatick nerve of a rabbit in the usual way, passing under it repeated doubles of very fine linen. I covered it with lint well steeped in the poison, putting bits of linen over the whole. The rabbit lived twenty-four hours, when it died suddenly, but I could not suspect its death to be caused by the poison.

I got ready the sciatick nerve of another rabbit as above, covering it with poison and bits of linen as usual. It died in forty hours without any symptoms of the disease of the poison.

I made the same experiment on the sciatick nerves of three other rabbits, taking the utmost care, after applying the poison, to have them well covered with linen, to prevent all suspicion of the poison's having soaked through. One of these rabbits died in three days; the other two were alive at the end of eight.

I got ready the sciatick nerves of two other rabbits exactly in the same way, but without applying the poison, for a comparative experiment. One of them died in thirty-six hours, the other was still living at the end of the eighth day.

I thought these experiments sufficient to determine whether the *ticunas*, applied externally to the nerves, is capable of producing any disease or derangement in the animal; but I wanted to know whether it would be equally inactive when applied to wounded nerves, or rather to the very substance of them.

Experiments with the TICUNAS on Nerves cut or wounded.

I prepared the sciatick nerve of a rabbit as above, and having pierced it several times through with a lancet, laid the poison exactly on the wounded part.
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The rabbit lived five days, when it died without any symptoms of disease. I repeated this experiment in all its circumstances on another rabbit, which was still alive eight days after.

I varied this experiment a little on the nerves of three other rabbits. Instead of making several wounds with a lancet, I made a longitudinal opening into the nerve of more than five lines in length, into which I introduced threads well steeped in the poison, and covered all securely. One died in sixty hours without any symptoms of the disease of the poison; the two others were living eight days after.

I thought it proper, again, to vary this second experiment, by cutting the nerves of a few rabbits, in the way I had done in examining the venom of the viper. I cut the sciatick nerve as far as I could from its origin, that so I might enclose it in linen. The part of it cleared in very large rabbits was about an inch and an half: having put linen beneath the nerve, I covered the cut part well with the poison, and placed linen over all as usual.

I made this experiment on six rabbits, two of which died in forty hours, two in three days, and two were still living on the fourth.

To make a comparative experiment, I prepared the nerves of two rabbits as above, but did not apply the poison. One died in thirty-six hours, the other survived the third day.

The uniform consequences of these experiments on the nerves, made me think any further ones un-

necessary ; and I believe those will agree with me, who are accustomed to make experiments, and are not prejudiced by ill-founded hypotheses. The American poison, as is here seen, in whatever way it is applied to the nerves, is not a poison to them, and does not produce in these cases any sensible derangements in the economy of a living animal : this is proved by immediate experiment. To suppose what we do not see, to believe what is contradicted by experiment, is substituting dreams to real objects, embracing error for truth, and adopting chimeræ for facts.

The American poison then, agrees with the viper's venom, in being quite innocent to the nerves, in whatever way it is applied to them ; like the viper's venom it kills in the smallest quantity, and that instantly, if introduced into the blood by the jugular vein ; and its action is altogether on the blood, let the principle or mechanism by which it causes death be what it will.

The effects the viper's venom produces on the blood are more clear and decided ; there is an unquestionable coagulation of it, not to be perceived in the cases of the *ticunas* : we however observe in this last a great change in the lungs, which become violently diseased.

The sudden death of an animal, when the poison is injected into its vessels is really surprising ; it scarcely seems to have had time to reach the heart. Neither can we conceive how animals with cold blood ; frogs, for instance, that exist with an
impeded

impeded circulation, can be killed by it; although it is true that they die much later than animals that have the blood warm. The blood or any humour, changed by a poison, may indeed gradually produce in these animals, more considerable derangements than are caused by an impeded circulation.

The death which immediately succeeds the introduction of the *ticunas* into the blood, may induce a suspicion that this humour contains a very active, subtil, and volatile principle, which escapes the nicest eye, and even the microscope. Agreeable to such an hypothesis, this principle seems highly essential to life, and to be particularly acted upon by the poison.

What may lead one to suspect that a very active and volatile principle does really exist in the blood is, that the viper's venom prevents its coagulation when 'tis drawn from the vessels, and on the contrary, produces it in the vessels themselves. One would suppose in the first case, that something had flown off from the blood, which exists in it when 'tis enclosed by the vessels.

This active and vital principle, agreeable to this hypothesis, may be considered as resulting from the whole animal economy, without excluding the nerves, which may even contribute greatly towards it.

But these are mere conjectures, more or less probable, and not demonstrated by experiment. We must stick to certain facts, in whatever way we explain

plain them: it is certain then, that the *ticunús* does not act on the nerves, and that it acts altogether on the blood.

Previous to my experiments, no one would have doubted its immediate action on the nerves, which was announced by every outward symptom. These symptoms then, which the physicians improperly regard as a certain proof that the disease is purely nervous, are equivocal, and may present themselves without the smallest nervous affection: the mere change in the blood is sufficient to produce them in an instant. The greatest physicians have regarded the diseases caused by the viper's venom and the American poison, as derangements in the nerves: it is now their business to examine whether other diseases, ascribed to the nerves, are not rather diseases of the fluids, and of the blood. There are grounds for this suspicion; the symptoms are equivocal, and the principles not generally demonstrated.

I do not pretend to deny but that diseases of the nerves may occur; this would be running from one extreme to the other. There are without doubt, diseases nervous in their origin, and many others which become so by the changes that take place in other parts, even in the fluids. The passions of the mind shew us the influence the nerves have on the body; but this does not prove that all the diseases ascribed to the nerves, are really nervous, and that the usual symptoms of these diseases may not be equivocal. Besides, it is certain that the
poisons

poisons we have examined, have no immediate action on the nerves, whatever may have been hitherto believed.

Some one may object, that probably the viper's venom and the American poison, act only on the extremities of the nerves, and on that account are innocent when applied to the trunks of them. But every thing serves as an objection to those who busy themselves in imagining difficulties; and it is very rare to find two things in every respect alike. For my part, it appears to me, that the internal substance of the great nerves, is the same as that of their extremities; and that the former are subject to pain as well as the latter. I do not create hypotheses which are not confirmed by facts.

I may have been deceived in some of the consequences I have deduced from my experiments, and may likewise have been deceived in some of the experiments themselves, although I have strove to succeed, and have searched for truth in an unprejudiced way. I do not doubt but that any one who will apply himself after me, to these researches, will find something to add, and even perhaps to correct. It is sufficient for me to have opened a road to new truths, and to be able to attest the veracity of the principal facts I have advanced.

The greater part of these experiments was made in the presence of my particular friend Dr. Ingenhausen, Physician to their Imperial Majesties, who has displayed in several publications the talents of a real naturalist. M. Tiberus Cavallo has like-

wife assisted at several of the most important of them. I conceived that the authority of two persons known to the learned, would give an additional credit to my experiments.

On the poisoned Arrows brought from the East Indies.

After I had finished my experiments on the *ticunas*, one of my friends at London procured me a number of arrows from the East Indies, on which I made some experiments, but neither greatly multiplied nor varied them, as well because the arrows were but few in number, as because they seemed to differ from the American poison, only in having less activity. This last circumstance was probably owing to their not having been so well preserved as those from the West Indies, which indeed seemed to be the case, or because the poison had been prepared many years ago.

I could not succeed in killing even small rabbits by applying this poison to the skin, scraped or slightly scarified; although I laid it on in a greater quantity, and upon larger portions of the skin, than in my experiments with the *ticunas*: when given inwardly, in twice or thrice the quantity in which I gave the latter, it produced no sensible change, even in rabbits that scarcely weighed a pound.

I pierced the skin of several animals with these arrows, leaving them sticking in it for whole days,
without

without my perceiving any poisonous affection. But on piercing the muscles, and leaving the arrows plunged in them, I soon found the effects. Several of the animals poisoned in this way, died with the same symptoms that are produced by the *ticunas*. They were several hours in sickening, so that this poison does not seem to differ essentially from the other. It exactly resembles it when examined with a microscope, when thrown into the eyes of animals, when tasted, and when chewed. It dissolves with greater difficulty in water, in which the greater part of it remains in an insoluble state.

The only inferences to be drawn from the particulars I have related are, that this poison communicated to the muscles, acts with far greater force than when applied to the skin; that it agrees very well with the other poisons, and serves still to persuade us that the immediate action of poisons is not on the nerves; since it is certain that the skin is more sensible than the muscles, and is quite interwoven with nerves.

Experiments made with the TICUNAS, after my return to Italy in 1780.

I forced an American arrow into the body of one of the snakes, called *anguis miliaris*, near its tail. The snake lived, and scarcely seemed benumbed

numbed by it. I repeated this experiment with a fresh arrow on the same snake, which neither died, nor suffered a great deal. These two arrows had been previously dipped in the *ticunas* softened before the fire to the consistence of honey.

I passed another arrow in the same state, through the tail of a snake: in four hours it was motionless, and as if dead. On pricking the body with needles, there were notwithstanding some marks of irritability, which at length disappeared, so that I judged the creature dead. In thirty-six hours it however began to move, and continued in a very feeble state for five days more. In its first condition 'twas apparently dead; in the second certainly alive: nothing has surprised me so much as this kind of return to life, in so large an animal, in which the suspension of all its vital motions continued so long a time.

I repeated this experiment afresh on another snake of the same kind, steeping the arrow in warm water before I forced it into its tail: I left it there twenty-four hours, without its producing any effect. Soon after I forced another of these arrows into its body, where I left it for twelve hours, but without effect.

I pierced one of the fore feet of a land turtle that weighed four pounds, with an American arrow, which I left there for half an hour. In another hour it scarcely seemed to live, and in two appeared quite dead. After an interval of ten hours, I removed with a sharp instrument the inner shell,
taking

taking care that the fleshy parts should be as little torn as possible. The heart was still, and I scarcely found any motion in the auricles. But in a little time the heart, together with the auricles, recovered all its force, and continued in action for six hours incessantly; the auricles kept in motion for two days, that is to say, as long as they were moistened by the blood which flowed from the neighbouring vessels.

I pierced the fore foot of a land turtle that weighed a pound and an half, with an American arrow. In eight minutes it could scarce move, and in a quarter of an hour was dead. When the feet and neck were stimulated, they discovered a slight degree of sensation. Having opened the thorax, I found the heart and auricles quite motionless. I touched the heart thrice, and it contracted itself once each time. On freeing it from the membranes, it began to move very briskly, and continued to do so for several hours. I covered it with the inner shell, and in twenty-four hours found it again motionless. I pricked it once with a needle, it contracted itself a single time; I pricked it again, it contracted itself afresh, and continued to do so once every time I pricked it. I left it exposed to the air for three minutes, and it then began to move of itself, continuing a very brisk motion for several hours. I covered it afresh with the shell, and on uncovering it four hours after, found it motionless. I left it in the air for a few minutes, and in a short time, and of itself, it recovered

vered its oscillations, which continued for six hours. I again covered it with the shell, and on uncovering it two hours after, found it without motion. I then covered it with water, which I kept on it for ten minutes, without its producing any change. I drained off the water, and the heart was scarcely left in the air a minute, when it began afresh to move briskly, and continued to do so for several hours. Lastly, I put it in the sun, where it soon dried. The auricles likewise dried in a degree, and all was then still. I now wet both heart and auricles; the former continued always motionless, but the latter began to move, and continued their motion for eighteen hours, when becoming dry, they lost it for ever.

This succession of motions confirms still more the doctrine I have established on the irritability of animal fibres (*a*), and shows that the air is a very active principle in awakening the sensibility of muscular fibres, and of the heart.

It cannot be doubted, but that the *ticunas* attacks the principle of irritability in the muscles, although it has no action on that of the heart. It agrees in this with the other poisons, which do not usually act on this muscle or on the intestines: the motion of these last continues, even after the death of the animal, and when the irritability of the other muscles is totally destroyed.

(*a*) De Legibus Irritabilitatis nunc primum sancitis.
Lucca, 1775.

F I R S T T R A C T

O N

CHERRY-LAUREL WATER.

TO close my researches on poisons, I shall relate various experiments I have made on cherry-laurel water, a poison which for some years has been celebrated in Europe. It does not yield to the most active amongst them, when the great derangements it causes in the animal economy, and the suddenness of its action on being given internally to animals, are considered. It not only produces the most violent convulsions and death, in animals even of a middling size; but if given in a smaller dose, the animal writhes itself, draws its head and tail together, and forces its vertebræ out in a way frightful to the view.

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In this state the convulsions and agitations of the body are very violent, and in the midst of them the animal at length meets with a speedy death.

If given to an animal as an injection, it equally produces convulsions and death.

With less than two teaspoonfuls of this water taken internally, I have seen middle sized rabbits fall into convulsions in less than thirty seconds, and die in a minute. If swallowed by animals in a large quantity, they die almost instantly without convulsions, and with their bodies entirely in a sunk and relaxed state.

When given in a small quantity, convulsions more or less violent succeed, and the body and extremities of the animal become lifeless: the hinder feet first lose their use, then the fore ones, and so on. When the animal can neither stir its legs or body, it continues to move its neck and head very well, raising the latter without difficulty, and turning it freely about. In this state the animal retains its smelling and sight, and though it does not stir its feet of itself, it draws them back when they are pricked or violently squeezed: a proof that they have not wholly lost their sensibility.

Cherry-laurel water then is a very strong poison, either taken by the stomach, or introduced into the body as an injection. Its action is so sudden and violent, that the animal gives symptoms of pain almost on the moment of its being swallowed. It is true that a small dose of it is innocent; that is to say, that a few drops of it given to a small
animal

animal which would be killed by the same quantity of the *ticunas*, produces no sensible complaint : but this does not make an essential difference betwixt the water of the cherry-laurel and the other poisons that are better known.

On distilling a great deal of water from a few of the leaves, I found it to be quite innocent ; it becomes more active if repeatedly distilled over again from the same leaves, but is not mortal. If instead of adding water to the leaves of the cherry-laurel, the distillation is made *in balneo mariæ*, the liquor that runs off is a very strong poison, and very sudden in its effects. This is what I have principally employed, and I doubt not but that it may be brought to such a degree of activity, as to kill in a small dose, in the same way with the *ticunas*. For this purpose the liquor that rises the first time should be distilled over again several times on fresh leaves, well wiped and almost dry. I believe that if it were evaporated over the fire, an oily concrete substance would be obtained, which would not only equal in activity any of the known poisons, but would probably far exceed them all. I reserve this experiment for another occasion, when I shall also speak of bitter almonds, and the degree of poison to which their water may be carried, when distilled dry.

We have seen that cherry-laurel water kills animals when introduced into the stomach or intestines ; let us now see the effects it produces when

VOL. II.

I.

applied

applied to wounds. One of the many experiments I made on this subject will here be sufficient.

I made a cut of about an inch in the skin of a large guineapig's belly, and slightly scarified the bared muscles in several places, applying to them two or three teaspoonfuls of the water mentioned above. In less than three minutes the animal fell into convulsions, and died soon after. This experiment shows that the cherry-laurel water agrees with the other poisons in its action on the body, when introduced through the medium of wounds.

The success of this experiment was the same on other animals with warm blood; I however observed, that the water of the cherry-laurel acts invariably with greater quickness and force, when taken internally, even in a smaller quantity.

This last circumstance deserves, in my opinion, the greatest attention, since it is a determined fact, that a large wound has infinitely more vessels than the mouth or stomach, to absorb the poison almost at the very instant: besides which, the nerves contained in the wound, both from their number and the state they are then found in, ought to be very sensible to the action of this poison.

Not only animals with warm blood die very suddenly when made to swallow this water, but those with cold blood die too. What appeared to me very singular, since it is quite different with the other poisons, is that they die in a very short time, perhaps even quicker than the first. It will be
sufficient

sufficient just now to speak of eels, creatures very hard to kill, and the parts of which, when they are dead, continue to move a long time: they die in a few seconds after swallowing this water, contracting themselves at first, but becoming motionless the instant after their death, when their bodies are not even sensible to stimulation. The heart however continues to move, but not so forcibly as before; and this motion ceases much sooner than when they are killed by cutting off the head. It cannot here be denied but that the muscular irritability must be strongly acted on, and that in a particular way. All the animals with cold blood I have been able to procure, were killed by this poison, and I doubt whether any of them are capable of resisting it: if they are not, it ought on that account to be distinguished as the most terrible of all known poisons, from its universal faculty of destroying every species of animals.

But how does it kill in so short a space, when introduced into the stomach, where we perceive no vessels capable of receiving it? This difficulty requires some further experiments: we must see the effects it produces when applied immediately to the nerves, and when introduced into the blood without touching the parts that are cut.

I employed large rabbits, making my experiments on the sciatick nerve, as I had done with the venom of the viper and the *ticunas*. For brevity's sake, I shall relate here a single experiment, omitting all the others, which I do not think very ne-

cessary, after the many experiments on the nerves already related.

Having laid bare the sciatick nerve of a large rabbit, for the length of more than an inch and an half, I placed under it a piece of very fine linen, sixteen times doubled, to prevent the water from penetrating to the parts beneath it. I then wounded it lengthways several times with a lancet, and covered the wounded part, more than eight lines in length, with a bit of cotton about three lines thick, well steeped in cherry-laurel water, of which it absorbed more than fifteen drops: all of this was communicated in an immediate way by the wounds, to the medullary substance of the nerve, which I covered for several minutes with fresh linen, so that it was impossible for the poison to be communicated either to the parts beneath or those adjacent. Having made the future in the skin, and left the animal at liberty, it ran about, eat, and was as lively as before, and neither then nor afterwards, seemed at all affected by this poison, which kills so suddenly when taken by the stomach. This and several other facts, analogous to those of the viper's venom and *ticunas*, show us that cherry-laurel water, in whatever way it is applied, whether immediately to the nerves or even introduced into their medullary substance, has no action on them, either as a poison or otherwise.

After all the experiments related in this work on the venom of the viper, and on the American poison, which is still more powerful; after having
seen

seen that neither of these two poisons has any action, when applied in an immediate way to the nerves, whilst they kill the strongest animals the moment they are introduced into the blood; there cannot be a more natural inference than that the water of the cherry-laurel, innocent like the others when applied to the nerves, ought to be likewise destructive when introduced into the blood; and yet the case is altogether different: so true it is that we ought to mistrust analogies, even when they are most uniform.

I sat about introducing this water into the jugular vein of a large rabbit, beginning with five or six drops, as I had done with the viper's venom and *tig-nas*. The animal giving no symptom of pain, I thought I had not succeeded in the attempt, and that the syringe had found its way into the cellular membrane. I repeated this experiment, introducing afresh, perhaps three or four times the quantity of the water, and assuring myself previously that the point of the syringe had entered the vein, and that the liquor could in no way force itself back: the animal still continued to be unaffected by it. I was more surprised than satisfied at what I saw. I could not persuade myself but that the cherry-laurel water would be a poison, and even a very powerful one, when introduced into the blood, since applied to the nerves, it was quite innocent. I returned then to my experiments, and now introduced a teaspoonful of the water into the jugular vein, from which the animal felt no ill effects. I repeated this experi-

ment on another rabbit, and introduced the same quantity of the poison, the creature neither suffered at the moment nor afterwards.

The unexpected result of these experiments threw me into the greatest uncertainty as to this poison; I was not only puzzled at its mode of operation, but even as to the parts on which it acts, when taken internally or applied to wounds. It does not act perceptibly on the nerves; it has no action on the blood; and yet it kills when swallowed, and that in an instant. The deaths of animals may then be brought on in another way than by the blood and nerves! The loss of motion in a few seconds in creatures such as eels, which continue to move for whole hours after their heads are cut off and their bodies in pieces, would induce a belief that this poison acts on the irritability of the muscular fibres. It is true that in these animals the heart still continues to move, but its motion is much diminished, and of very short duration. In animals with warm blood, poisoned by the water of the cherry-laurel, a very feeble motion still exists in the other muscles, and if the heart continues to beat for some time, its motion is feebler than when they are killed in any other way. There is certainly a very great diminution of irritability in many animals, and in many others a total loss of it, in whatever way this loss or this diminution may contribute to bring on so speedy a death, and however obscure the mechanism may be by which this action is wrought on the muscular fibres.

We must avow our ignorance in searching into nature; when we think we have done every thing, we frequently find ourselves returned to the spot we fat out from. Experiment, which is the only guide we have in our researches, undoubtedly secures us effectually from falling into error; but it does not always draw us into an acquaintance with remote truths; neither does it always lead us to a knowledge of the secrets of nature, nor carry us invariably as far as we proposed to go.

But if we do not know how the cherry-laurel water acts, and on what parts it exercises itself in destroying animals, we however know that when it is applied immediately to the nerves, and even to the medullary substance of them, it is quite innocent: and it is not less true, as the many experiments hitherto related have clearly demonstrated, that the viper's venom and the *ticunas* are not mortal in whatever way they are applied to the nerves, but are always so when introduced into the blood. These truths, which we were formerly ignorant of, are now cleared up in such a way that they cannot be called in doubt, and destroy all the systems which writers have invented on the action of these poisons: it is from these facts that we must proceed to an intelligence of them, and of their action.

The applying the water of the cherry-laurel to the different parts of the brain of a living animal may probably throw some light on its action; I postpone the doing it till I have more conveniences for that purpose than at present, and shall then, I hope, fur-

nish some new and more interesting particulars as to this poison, and the parts of an animal on which it acts when it destroys it.

I reserve to this opportunity the examining whether it acts on the lymphaticks, or rather on the lymph itself. I suspected this after trying the experiments I have recited, but could not then enter into an investigation of it. My experiments on cherry-laurel water are consequently less complete than I wished: They must be multiplied and pursued more than I have been able to do, and this is still another reason that I continue my researches on this truly interesting subject.

On the action of Poisons on the Nerves.

It has been seen in the course of this work, that the venom of the viper, and the *ticunas*, in whatever way they are applied to the nerves, are innocent; and on the contrary, that when they are introduced into the blood, without touching the wounded parts adjacent, they bring on a sudden death. It has likewise been seen, that these two poisons throw an animal into very violent convulsions, and excite the most decided symptoms of those diseases, which the physicians call nervous, because they believe them to arise simply from a nervous affection. It appears, that there can be no doubt in future of these poisons being absolutely innocent when in immediate contact with the
nerves;

nerves ; and that they have no immediate action on the solid parts, such as the muscular fibres, the bones, cellular membrane, and tendons. But this is not sufficient to lead to a perfect knowledge of them. The nerves are certainly excluded in the diseases they occasion, and the blood alone seems to be acted on. But how many different humours do we not find mixed with the blood ? It has even been believed, that it is animated, and it seems more than probable that the nerves are perpetually secreting a humour which is mixed and circulates with it. May not this humour be essential to life, and be attacked by these poisons, when they are introduced into the vessels ? In reply to this query, frogs are known to live, as the learned Spalanzani has observed, even after their vessels are emptied of the blood, and in this state die very readily, if made to swallow the spirit of the cherry-laurel, as I have observed several times. Thus is the first of these two hypotheses proved to be false, and the second is by no means sufficient to explain the action of this poison on the parts of animals.

The spirit of the cherry-laurel, which acts but feebly when applied to wounds, which is innocent when applied to the nerves, and which kills when simply applied to the mouth and eyes, throws us into fresh uncertainties, and scarcely leaves room for conjectures. A substance which is a poison in the stomach, in the mouth, and in the œsophagus, and which is almost innocent when applied to wounds, is a true paradox, and reiterated experiments are
scarcely

scarcely sufficient to convince us that the fact is real.

The only sure consequence to be drawn from so many experiments is, that an animal may have all the symptoms of a nervous disease, without our being able to assure ourselves that the nerves are affected. The convulsions likewise that are observed in animals from the diminution of the blood alone, or the unequal distribution of this fluid in the different parts, in the way I have demonstrated in several parts of my different works, leave no room to doubt it (*a*).

I have only to wish at present, that some skilful physician, gifted with the rare talent of nice observation, and divested of all prejudice for hypotheses that want the necessary support of facts and experiments, would find leisure to examine with a critical nicety this important subject, which constitutes at this time one of the principal branches of modern medicine. Such an investigation may at length decide with certainty, whether all the diseases that are stiled *nervous*, and which are said to originate from a change in the system of nerves, have really such an origin; whether there are characteristick symptoms, invariable and constant, of this species of diseases; whether there is a certain criterion, a sufficiently faithful touchstone, to assure

(*a*) This first tract on the cherry-laurel, and likewise, the treatise on the *Ticunas*, were printed in the 68th volume of the Philosophical Transactions.

us of it; and whether the greater part of these diseases may not be caused by the blood, or other vitiated humours, rather than by the nerves. Is the perceiving the nerves to be in some measure affected, really sufficient to characterize a *nervous* disease? Can we determine a disease to be *nervous*, because several of its effects manifest themselves on the nerves? An affection of the nerves may very easily be attached to a particular class of diseases, and yet the nerves not be the cause of them; ought we thence to say that such diseases are *nervous*? We do not here demand sublime and abstracted theories, which a singular effort of genius frequently gives birth to: we are in need of nice observations; new and well imagined experiments; direct and useful inductions, drawn by a calm mind, and one capable of assembling and combining in the best manner, the most luminous particulars. Three of the most learned physicians of our days have by their writings fully satisfied the first of our demands; we have now to desire that a fourth will at length finish this important work, by applying himself assiduously to the last.

S E C O N D T R A C T

O N T H E

C H E R R Y - L A U R E L.

SHORTLY after my return to Florence in 1780, I had an opportunity of re-examining the effects of the spirit of cherry-laurel on different animals. I therefore thought it proper to give a greater extension to my experiments on this poison, than I had done in England; and my labours have not altogether been without success. I have at least established truths I was before ignorant of, and have excluded many useless or false hypotheses, which might have retarded the researches of those who may wish to busy themselves hereafter on this subject. I very justly observed some time ago, that in distilling the leaves of the cherry-laurel without water, a spirit was obtained capable of
killing

killing animals in a very short space, although it was given to them only in a small dose. I likewise observed that if water was put on the leaves, the spirit would become altogether innocent, and preserve nothing but an agreeable flavour. But I did not know whether the essential oil drawn from the cherry-laurel by distillation was innocent, or acted as a poison; and whether, supposing it hurtful, it was still more so than the spirit. I likewise did not know whether the deadly effects of this spirit were caused by its having in it more or less of this oil in a dissolved state. It was owing to the want of experiments that I was ignorant of all these particulars, and of many others which the authours who have written on poisons have not examined into. None of them, that I know of, have made experiments on the *empyreumatical oil*, and on the *extract* of the cherry-laurel; neither have I met with any one who has made direct experiments on the greater part of the above particulars, to serve me as a guide, and yet they appear to me necessary to the right understanding the nature and qualities of so very singular and active a poison.

To proceed methodically, I shall give a concise detail of the different productions I have drawn from the cherry-laurel, and of the method I pursued for that purpose. In distilling the leaves in the usual way in glass retorts, and without adding water, I obtained the spiritous part, (what the chymists call *rectified spirit*). This spirit was transparent, fragrant, poignant to the taste, and at the
bottom

bottom of the receiver there was a certain quantity of a heavy, coloured, odoriferous oil, which was bitter, pungent, and burning to the taste. I separated [this very attentively from the spirit, after letting it settle for several days. I likewise employed the spirit not entirely deprived of the oil. I shall call this oil, *oil of the first distillation*, and the spirit, *spirit of the first distillation*.

I took a quantity of the spirit of the first distillation, and distilled it afresh till there remained about one third in the retort. The spirit came off more transparent, fragrant, and poignant, more bitter and pungent, than that of the first distillation. Being left in a bottle to settle, it deposited a very transparent, odorous, burning oil, very like in its external qualities the oil of the first distillation. I shall call this, *oil of the second distillation*, and the spirit, *spirit of the second distillation*. What remained in the retort I shall call *residuum*, or *phlegm of the second distillation*. I prepared another phlegm by evaporating two thirds of the spirit of the second distillation in the sun.

I mixed a quantity of the spirit of the second distillation with the same quantity of decrepitated and well dried spirit of sea salt. I distilled this mixture with a slow fire, and drew off one half: this was of the colour of common oil, and less spirituous, pungent, and fragrant than before. It precipitated an oily coloured substance, of an earthy appearance, and divided into small grains or globules. I continued the distillation, and there came
off

off a phlegm without smell, and not sensibly pungent, although put on the tongue, or rubbed on the nostrils. I shall call it *phlegm of the third distillation*. The oil I have just spoken of, I shall call *oil of the third distillation*, and its spirituous part which came off the first, *spirit of the third distillation*. Both oil and spirit have the smell of bitter almonds.

I drew off in the same way the *extract* of the leaves of the cherry-laurel, pursuing the steps laid down by chymists; and likewise prepared a good quantity of *empyreumatical oil* with fresh leaves. After having furnished myself with all these preparations, I began my experiments on animals, chusing rabbits, guineapigs, pigeons, frogs, and adders. I thought it necessary to try them on animals of different natures, some with warm blood, and some with cold, because I knew by experience how much the action of poisons varies in different animals, and above all in the larger species of them, in which the economy of the various movements is so different.

Spirit of Cherry-laurel of the first distillation, given internally.

I let this spirit settle for some days to separate the oil from it, so that it became clear and transparent. I gave a teaspoonful to a pigeon of a middling size,

which in four minutes fell into convulsions, and was for some time not able to support itself; it however survived.

I gave three teaspoonfuls to a small guineapig, which felt no ill effects from it.

I gave two teaspoonfuls to a pigeon, which in a minute could not support itself; in another minute it fell into strong convulsions, and died on the third.

I repeated all these experiments on the following day, on the same animals, and the result of them was the same.

As the pigeon died which swallowed two teaspoonfuls of the spirit, I wished to try it before the oil had sunk to the bottom. In this state it is less clear, more pungent, and more fragrant. The want of activity in the spirit employed above, arises, as will be seen, from my having left the oil to precipitate for several days, and in reality that which I had employed in London was thick, and acted with far greater violence.

I gave then to different animals, such as rabbits and guineapigs of a middling size, three teaspoonfuls of the spirit in the state I have spoken of, and the greater part of them died convulsed in a very little time: a proof that this spirit is much stronger than the other.

Spirit

Spirit of the second Distillation given internally.

I gave to a small guineapig a teaspoonful of this twice rectified spirit: it died almost instantly.

I made a large rabbit swallow a spoonful of this spirit. It immediately fell, and died a short time after.

I gave four drops to a guineapig of a moderate size. In the very act of deglutition, a liquor of a yellow and green hue poured from its mouth. This frequently happens when the spirit is swallowed by these animals, but never when they swallow the oil. It was otherwise without complaint.

I gave a large rabbit six drops of this spirit mixed with forty drops of water. The creature lay down several times on its belly, seemed very uneasy, but did not die.

I gave four drops to a pigeon, which expired in less than a minute.

I gave four drops to a frog, which in two minutes was to appearance dead, and the parts of which in two more, although stimulated, were totally without motion.

Phlegm of the Spirit of the second Distillation.

This phlegm was scarcely spirituous or pungent, but was very transparent.

I gave it to several animals, such as pigeons, rabbits, and guineapigs. The pigeons to which I gave a teaspoonful all died : of those to which I gave only a few drops, some had no complaint, some died after a long time, and others were only convulsed.

A few of the very small rabbits and guineapigs died, others were convulsed, and others again had no sensible complaint. Those which died, had swallowed three or four teaspoonfuls.

The phlegm then is less destructive than the spirit.

Phlegm of the Spirit of the second Distillation, obtained by evaporating two thirds of it in the Sun.

I put about three ounces of the spirit of the second distillation in the sun to evaporate. The residuum being an ounce, was liquid, transparent, and almost without smell : when put on the tongue it was still pungent, although much less so than before. I gave half a teaspoonful to a pigeon, which fell instantly into violent convulsions and died. Five other pigeons died in the same way. These experiments would lead one to suppose, that the poison neither consists in the fragrant nor the pungent quality of the cherry-laurel, since both of them were so trifling, and the animals notwithstanding died so suddenly.

I gave a teaspoonful of the same phlegm to two pigeons, both of which immediately died.

I gave

I gave three drops to a pigeon, without its having any effect, whence we may infer that this phlegm is also less destructive than the spirit:

Spirit of the second Distillation applied to the Mouth.

I wished to know whether this very active and destroying spirit would kill, when simply applied to the inner part of an animal's mouth.

I wetted a piece of linen with it, and introduced it into a pigeon's beak, without the possibility of a drop of the spirit passing into the stomach, or even the œsophagus. In thirty seconds the pigeon fell into convulsions, and died immediately.

I wetted another piece of linen with it, and kept it a long time in the mouth of a middle sized guineapig, which was not annoyed by it.

I repeated the same experiment on two pigeons, both of which died in less than two minutes.

I repeated it on two guineapigs, which continued free from complaint.

This spirit then is capable of killing weak animals without touching the œsophagus or stomach.

Spirit of the second Distillation put on the Eyes.

It remained to know whether this spirit would be likewise destructive, when applied to other tender parts of the body. Here it occurred to me to make my experiments on that very sensible organ, the eye.

I let several drops of the spirit fall on the naked eyes of a guineapig. It suffered severely; but had neither convulsions, inflammation, nor any other symptom of poison.

I made the same experiment on the eyes of two other guineapigs, and the consequence was the same. I repeated it on the eyes of two rabbits, but although the spirit clearly incommoded them, they neither died, nor were convulsed; and their eyes were not apparently inflamed.

These experiments do not yet prove that the spirit of the cherry-laurel is entirely innocent when applied to the eyes of these animals, because they are hard to kill, and make a strong resistance to the action of the spirit, when it is simply applied to the mouth.

It is true that I covered the eyes of two very small guineapigs which weighed only three ounces each, with the same spirit, and repeated the application of it more than twenty times, but in vain: they had no symptom of poison, and their eyes were free from inflammation, although they were very sensible of pain when I applied the spirit to them.

But I thought it would be right to make some experiments likewise on pigeons. I moistened a pigeons eyes several times with a piece of linen wetted with the spirit: after a short time it vomited repeatedly, and fell on its breast. The iris in the vicinity of the transparent cornea was a little inflamed, the pupil moveable, and of its natural size.

I let

I let fall some drops of the spirit on the eyes of another pigeon, keeping them on the part for more than two minutes; it was seized with convulsions, and died in a few instants, without the eyes being inflamed.

I put several drops of the spirit on one of the eyes of a third pigeon, keeping it on for three minutes. The iris was much inflamed, as were the eyelids in a degree. A short time after, the pigeon was seized with convulsions, and appeared as if dead. In a quarter of an hour, it recovered by degrees, and at length seemed quite well; however it fell afresh into convulsions, appeared for a second time as if dead, and very soon after revived again. The iris of the eye to which I applied the spirit, was as red as if it had been injected, the pupil was immoveable and much enlarged: the iris of the other eye was likewise a little red, the pupil in its natural state. After the animal's relapse and second recovery, the pupil and iris were in the state they were the first time; but after the third attack, from which it recovered itself perfectly, the pupil became moveable like the other, the inflammation of the iris was much lessened, and both pupils recovered their natural size.

I let fall several drops of the spirit into the eye of another pigeon, and kept them there for several minutes: the creature fell into convulsions, and could not support itself. The iris was inflamed; that of the other eye was a little so, but infinitely less. The pigeon recovered by degrees, and I then found the iris immoveable, enlarged and inflamed; the other

was moveable in the light, a little inflamed, and of its natural size. The pigeon fell insensible three times, and as often recovered. The pupils and irises, which were affected as I have described, came to their natural state in a few hours.

Spirit of the second Distillation applied to Wounds.

It was natural to conceive that this spirit would kill still more readily, when applied to wounds. I made large incisions in the legs of a pigeon, and introduced a great quantity of it, but the creature had no symptom of complaint.

I repeated this experiment on two other pigeons, and the result was the same.

I applied a piece of linen wet with the spirit, to the wounds of two others, and left it there several minutes: the pigeons were not disordered.

I wished to see whether it would be as inactive, when applied to the pectoral muscles. Having laid them bare, and wounded them in several places, I immediately applied the spirit, and fomented them with linen wet with it. The pigeon had not the smallest complaint.

On repeating this experiment on three other pigeons, the event was the same.

I was on the point of concluding that the spirit of the cherry-laurel, in whatever way it is applied to wounds, is neither poisonous nor mortal, although it is highly so when put on the eyes, or applied to
the

the mouth. The singularity of this particular induced me to continue my experiments, and they convinced me that I had been deceived.

I raised a large portion of the skin of a pigeon's breast, and applied about an hundred drops of the spirit. A short time after the pigeon fell into convulsions, and died.

I laid bare the muscles of the leg of another pigeon, and wounded them in several places. I bathed the wounds repeatedly with the spirit, and kept them wet with it for eight minutes. The pigeon did not suffer during this interval, but two minutes after, fell on its breast and died.

I made a large opening in the skin of a guineapig's back, and made several superficial wounds in the muscles: I introduced at the opening in the skin, a great quantity of the spirit, and that repeatedly for several minutes. It was at first slightly convulsed, soon after it fell on its breast incapable of supporting itself, and died in a very little time.

I laid bare the muscles of a pigeon's breast, for a great length, and made several deep wounds in them. I applied the spirit in such a way that it could not reach the skin, and renewed the application to the wounds more than thirty times, keeping it applied for at least twelve minutes. It at length vomited, then fell into convulsions, and died very soon after.

It is an established truth then, that the spirit of the cherry laurel kills when it is applied to wounds, although it is likewise true that it acts much later than when applied to the eyes, and mouth, and re-

ceived into the stomach, in which cases a smaller dose is destructive. It is very singular, and still as certain, that the quantity which kills an animal, a pigeon for example, in the latter cases, brings on no sensible complaint when it is applied to wounds.

Spirit of the third Distillation,

I gave half a teaspoonful of this spirit to a large guineapig, which did not suffer sensibly from it ; but three pigeons, to each of which I gave scarcely three drops, died ; as did also three rabbits and four guineapigs, to each of which I gave a teaspoonful. A large guineapig and a large rabbit survived however, after swallowing this quantity, although each of them was visibly disordered.

*Spirit of the Cherry-laurel of the third Distillation,
made by mixing a quantity of decrepitated Sea Salt
with the Spirit of the second Distillation.*

This spirit has scarcely any smell, and is almost insipid. I gave not quite a teaspoonful of it to a pigeon, which instantly fell into convulsions, and died soon after. Two other pigeons died on swallowing a much smaller quantity ; so that the sea
salt

salt does not seem to have changed the natural qualities of the spirit.

Phlegm of the third Distillation, with scarcely either smell or taste.

I gave three drops of this phlegm to a pigeon, which did not suffer from it.

I gave a teaspoonful to a large guineapig; it fell in a moment into convulsions, but afterwards raised itself and continued well.

I gave the same quantity to a guineapig of a middling size, which felt no ill effects from it.

I gave a teaspoonful to a very small guineapig, which continued well.

I made a large guineapig swallow a spoonful and an half. It vomited a little foul matter, but recovered.

I gave the same quantity to a small guineapig: it fell instantly into convulsions, but shortly after recovered itself perfectly.

I gave a spoonful to a pigeon, which fell immediately into convulsions, and died in less than a quarter of an hour.

Oil of the Cherry-laurel given internally.

It remained for me to examine the oils of the cherry-laurel, and after having assured myself by reiterated

reiterated experiments, that there is no essential difference between them, whether they are of the first, second, or third distillation, I did not think it necessary to distinguish them, and have therefore employed them indifferently. What was of the most consequence to me was, to know whether this oil is likewise a poison, and if so, whether it is stronger or weaker than the spirit. I shall on that account relate some of the experiments I made with it on different animals, and which will suffice to decide as to its poisonous qualities, and to show the frequent anomalies which result from the same subject.

I made a large rabbit swallow two drops of the oil, united with two drops of spirit. It died in a few seconds, slightly convulsed.

I made a land turtle of a pound weight swallow about two drops of pure oil. Two hours after, it was become very feeble : in somewhat more than six hours it died with all the symptoms of a loss of irritability.

I gave a large guineapig four drops of the oil, from which it felt no ill effects.

I gave scarcely three drops to a pigeon, which died in two minutes.

I made a large guineapig swallow half a tea-spoonful. It remained well for more than half an hour, but afterwards fell into violent convulsions, and died half an hour after.

I gave

I gave a pigeon a third of a teaspoonful of this oil; in a very little time it could not support itself, and died in less than half an hour.

I made a large guineapig swallow about six drops of the oil mixed with forty drops of spring water. It immediately gave symptoms of uneasiness; had several reachings to vomit; but a short time after became well and continued so.

I gave a small guineapig six drops with forty drops of water. It was very uneasy; but neither fell, was convulsed, nor died.

I made a frog swallow three drops, and in two minutes it was quite dead: the heart still preserved its motion, and the feet stirred, on stimulating the crural nerves.

This experiment was repeated on two other frogs with nearly the same consequences.

Notwithstanding the little conformity in all these experiments, we may conclude that the oil of the cherry-laurel is a violent poison, and kills both animals with warm blood, and those with cold. We may determine it to be far less active than the spirit, and that accidental circumstances, and a diversity in animals, are sufficient to prevent its noxious effects. It is very strange that it kills an animal with cold blood much quicker, as we have seen, than one that has the blood warm.

Oil of the Cherry-laurel applied to the Mouth.

I wished to see whether the oil of the cherry-laurel, which is a violent poison when received into the stomach, would be so too when only introduced into the mouth and palate, without entering the œsophagus. The experiments I have related on the spirit of the second distillation, led to a suspicion that this may be the case.

I made a piece of linen a little moist with oil, and introduced it into the mouth of a very small guineapig. I prevented it from shutting its mouth, to avoid the linen's being compressed so as to force the oil into the stomach. After keeping it in this state for two minutes, the animal was as well as before.

I repeated this experiment on another small guineapig, and rubbed the inner part of the mouth several times with the linen. The animal soon became very dull; but neither died, nor was convulsed.

I repeated this experiment on two other pretty large guineapigs, neither of which discovered any symptom of complaint: but these experiments are not decisive, because these animals are very hard to kill, and because a greater quantity of the oil was perhaps necessary. I had recourse then to pigeons, which are so easily killed.

I wet

I wetted a piece of linen with the oil, and introduced it into a pigeon's beak, in such a way that it could not reach the stomach or even the œsophagus. The pigeon died soon after.

I repeated this experiment on four other pigeons, three of which died very soon. The fourth had scarcely any complaint.

I think that we may conclude that the oil of the cherry-laurel acts as a poison, even when it neither touches the œsophagus nor stomach; and that for this end it is sufficient for it to be in contact with the inner part of the mouth.

The consequences of these experiments are entirely analogous to what we have seen above in making use of the spirit of the cherry-laurel.

Oil of the Cherry-laurel applied to Wounds.

We can no longer doubt but that the oil of the cherry-laurel is a poison, and a very violent one too, when taken internally. It now remains to know, whether it is a poison when applied to the wounded parts of an animal. Experiments alone can decide this, and those we have already made on the spirit of the second distillation lead us to presume, that it is likewise a poison in these circumstances.

I thrust a bit of wood dipped in this oil. into a pigeon's leg, and observing that after fifteen minutes the creature was not disordered by it, I took

it

it out, and introduced a great deal of the oil into the wound, which was very deep ; the pigeon notwithstanding, neither died, nor was convulsed.

I made a wound in the body of a small tortoise, towards its tail, and introduced the oil freely ; the tortoise was not disordered by it.

I made a wound in a pigeon's leg, bathing it several times with the oil, with which, put on linen, I likewise covered the wound. The pigeon was not disordered by it.

I wounded the legs of a pigeon in several places, and rubbed the wounds with this oil. It did not suffer sensibly.

The consequences of experiments on two other pigeons, three rabbits, and four guineapigs, were the same, notwithstanding that I did not spare the oil, with which I covered the wounds repeatedly, as I had done after wounding the muscles of these animals.

I wounded the pectoral muscles of three other pigeons, and covered the wounds with the oil ; neither of the pigeons were at all disordered by it.

It appears beyond a doubt, that the oil of the cherry-laurel, which is a poison when swallowed, has not this quality when applied to wounds, of the parts at least, on which I made my experiments : this is quite contrary to the nature of the viper's venom and other poisons, which are innocent when swallowed, and destructive when applied to wounds. If any thing probable can be advanced to account for this difference betwixt
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the oil of the cherry-laurel, and the venom of the viper, I do not see what can explain the variety of action in the former, on the different parts of an animal; this is a very singular circumstance, and what one would least expect. I must however confess that my experiments are not altogether decisive, although I made them on pigeons; because I did not employ the same quantity of the oil, as I had done of the spirit. I was in want of this substance in the greater part of my experiments, and have not till now had conveniences to make it afresh. Notwithstanding this, it is singular that what is poisonous when taken inwardly, should be innocent when applied in a much greater quantity to wounds.

Oil dried in the Sun.

I placed about two drachms of the oil of the cherry-laurel in the sun to dry, till about half was wasted; the residuum was yellow, bitter, fragrant, and pungent. I gave about three grains of this in weight, mixed with twenty drops of water, to a pigeon, which fell a moment after, was convulsed violently, and died very soon. I repeated this experiment on three other pigeons, and the consequences were the same. It is therefore certain, that this concrete residuum is a strong poison, and
that

that the evaporating a part of it in the sun, does not at all deprive it of its hurtful qualities.

The residuum of the oil of the cherry-laurel dried in the sun, is a true resin; when precipitated from spirit of wine, by the means of water, it is no longer poisonous.

It has been seen that the concrete part of the oil of the cherry-laurel exposed to the sun, is a strong poison. It does not dissolve in water, but is easily and perfectly dissolved in spirits of wine, It is a resinous substance then, which retains a deleterious quality. To know whether it would still retain it, if dissolved in spirit of wine, and precipitated by water, I threw a great quantity of distilled water on this solution of it, and when it had sunk in the form of a white farinaceous powder, I washed it in several waters. It had scarcely preserved any smell, but when put on the tongue, and chewed, it was still pungent. I gave twenty grains of this substance, whilst still wet, to a very small guineapig, and as much to a pigeon; neither of which either died or felt any ill effects from it.

I repeated this experiment on two other guineapigs, and the event was the same. It follows then, that this resin, after being dissolved in spirit of wine, and precipitated by the means of water, becomes innocent, although it preserves in a degree its smell and pungency; and it appears that this smell which is left, together with the pungent and caustick quality, is not sufficiently deleterious,

to kill, or produce any sensible derangements in animals.

Extract of the Cherry-laurel.

I made a guineapig swallow about thirty grains of this extract; it had no effect on it.

I made the same trial on a rabbit, which felt as little.

I gave about fifteen grains to a pigeon, without any sensible effect.

I repeated this experiment on two other pigeons, with the same success. We may infer from all these experiments, that the extract of the cherry-laurel is quite innocent.

Empyreumatical Oil.

I made a guineapig swallow about twenty drops of the empyreumatical oil of the cherry-laurel; it vomited soon after, but speedily recovered itself, and continued well.

I gave a small pigeon twelve drops of this oil. It vomited several times, was very feeble, but soon recovered.

I gave about thirty grains to a rabbit. It vomited repeatedly, but was soon re-established.

I gave to two pigeons twenty drops each ; they vomited several times, were very weak, but neither died nor were convulsed.

Two guineapigs and three rabbits had the same vomitings, but neither of them died nor suffered a great deal.

We may conclude from hence, that the empyreumatical oil of the cherry-laurel, is rather an emetick, than a poison, since it does not kill even the most delicate animals, although exhibited to them in a very strong dose.

From what has hitherto been said, the following facts may be deduced.

1st, That the spirit of the cherry-laurel is a poison.

2^{dly}, That the oil of the cherry-laurel is likewise a poison.

3^{dly}, That the spirit of the cherry-laurel, almost entirely deprived of its smell and pungent taste, is likewise a poison.

The poisonous qualities do not seem to consist in the fragrant and pungent particles, and this is further proved by the oil, dried, and afterwards dissolved in spirit of wine and precipitated from that menstruum, which is still fragrant and pungent to the taste, but is no longer a poison.

Besides, the dried oil is a true resin; and continues to be a poison even in that state. The deleterious principle then in this resin is destroyed by the spirit of wine.

As

As a little oil and some fragrancy remain in the spirit of the cherry-laurel, even when dephlegmated, the deleterious principle of this spirit may be the same with that of the oil, and that which is found in the resin.

It is not besides proved, that the oil is more powerful and active than the spirit. It has not however appeared invariably so, when given to animals.

It is true that we find the spirit of the cherry-laurel to be less active than before, when it is evaporated, and reduced to a third, or deprived of its most spirituous parts by distillation; in this state it has lost a good deal of its original smell and taste, which may be said to contribute in a great measure to its poisonous qualities: but on the other hand, after the oil has been precipitated by the spirit of wine, it still preserves its taste and smell, and is however no poison.

So that after all these experiments, notwithstanding that they have been greatly varied and multiplied, we are ignorant in what the poison of the leaves of the cherry-laurel really consists; we are ignorant of the mechanical action of this poison; and we are ignorant on what part of an animal it acts, when it causes its death; we have notwithstanding learned by our experiments, and by the particulars we have related, many truths we had no previous knowledge of, and which we could not have divined: thus is the knowledge of man always accompanied by ignorance. We do not

seem permitted to go beyond simple experiment, and it is to that alone that we ought to confine ourselves. But how many cases are there where either experiments are silent, or where we cannot succeed in conceiving any thing decisive from them!

What however deserves our principal attention is, the seeing that this poison, when simply applied in a very small quantity to the eyes or to the inner part of the mouth, without touching the œsophagus, or being carried into the stomach, is capable of killing an animal in a few instants; whilst applied in a much greater quantity to wounds, it has so little activity, that the weakest animals, such as pigeons, resist its action.

This circumstance appears to me a very particular one, and deserving of a further and very nice investigation. I do not despair of an opportunity of returning to the subject, and shall then endeavour to give a greater certainty and extension to my other experiments on this poison, particularly to that of injecting the water of the cherry-laurel into the veins of a living animal. I found it quite innocent to the few animals on whom I tried it in this way in London, in small doses: this is very different from the action of the other poisons.

EXPERIMENTS

ON SOME OTHER

VEGETABLE SUBSTANCES.

On the Toxicodendron.

I WAS desirous of making a series of experiments on the Toxicodendron, which the most celebrated writers have in general regarded as a very strong poison, although some modern naturalists have not found it so to certain animals. But I was forced to abandon my enquiries into this plant in the very beginning, having had the misfortune to poison myself three times successively with the leaves of it. Thus have I dearly paid for my scepticism and want of precaution, in becoming, myself, the subject of my experiments.

I began by expressing the juice of the leaves, which I made several animals swallow; they neither died nor fell sick, although they swallowed a pretty large dose of it. I likewise gave it in the form of an extract, and this preparation had no more effect than the other; the person indeed who gathered the leaves, had a complaint which very much resembled an erysipelas, particularly in the beginning. Whilst he detached the leaves from the plant, two very small drops of their milk fell on the back of his hand. Three days after, two small dark spots made their appearance at the place where the milk had fallen, and in three days more, his whole face, eyes, and neck, began to swell, becoming red and inflamed; as did also his breast, and hands. He had no fever, but was nevertheless obliged to keep his bed for upwards of fifteen days, and the epidermis fell off by degrees in small portions, causing a continual troublesome sensation, at once of smart and itching. It seemed to me very strange that so small a quantity of this milk should occasion so extensive and troublesome a complaint, and that it acted so late. Other poisons have no action when applied to the epidermis, at least none that has been observed. I was inclined to think, I must confess, that it was an accidental complaint, arising from some other cause.

I was still more confirmed in this opinion, on applying a great deal of this milk to the bared skin of several rabbits, guineapigs, and pigeons, after having made these animals swallow it mixed with the
crumb

crumb of bread, and after I had at length applied it to wounds I had purposely made in the skin and muscles. I could not perceive on any of these trials and experiments, that it created the slightest inconvenience to the animal. I was at length fully grounded in my persuasion, on letting several large drops of this milk fall on the hands of two gardeners, who indeed at the end of three days had the black spots I have mentioned, but were not sick. I was no longer afraid to make the same experiment on myself. I scarcely touched the back of my hand with a leaf of the toxicodendron which I had cut near the stalk, and could with difficulty perceive the skin to be wet at the place where I had applied it.

Three days after there appeared a dark spot, and in three days more, my whole face began to swell, particularly the eyelids and tips of the ears. I experienced a terrible smart for fifteen days, and an intolerable itching for fifteen more; even the hands smarted and itched, above all betwixt the fingers, which were become red, and were covered here and there with small vesicles filled with a transparent and sharp humour: I had no fever, but my pulse was very quick. The skin of my face, particularly about the eyes and eyelids, was extended and filled with an aqueous fluid, and easily retained the print of any thing that touched it. The epidermis fell off in small scales, and I felt a most troublesome itching through the whole process of the disease.

At the end of some days, and when I thought myself perfectly recovered, I was making experiments

on the air of the leaves of the toxicodendron, and whatever precaution I took, could not avoid touching some of the leaves with my fingers; this was in the parts where they had not been cut, and where there could be no suspicion of there being any milk. Six days after, all the parts that had swelled the first time, swelled a fresh, although not so violently, nor did the swelling last so long. My eyes and eyelids however gave me a great deal of pain, and got well the latest.

After an interval of twenty days, I wished to examine the air of some leaves of this plant, which I had caused to be got ready by another person, and I touched a few of the leaves when under water. In four days, my face and eyes swelled a third time, although not so violently as on the two first.

It cannot be believed after this that the milk of the leaves of the toxicodendron is innocent when applied to the human skin; but on the other hand, it is very singular, that an atom of this poison produces in a few days such remarkable symptoms, and in such remote parts, whilst the juice and milk of the leaves are quite innocent to animals, as well externally as internally, and even when applied to wounds. Its not acting on the two gardeners, was certainly owing to their hands being extremely callous, and I did not think it proper to apply it to the parts where the epidermis was more delicate; this first trial was however sufficient to assure me that parts which are become callous, resist this poison.

E X P E R I M E N T S

With the Oil of Tobacco.

I made a small incision in a pigeon's leg, and applied to it the oil of tobacco. In two minutes it lost the use of its foot.

I repeated this experiment on another pigeon, and the event was exactly the same.

I made a small wound in the pectoral muscles of a pigeon, and applied to it the oil of tobacco; in three minutes the animal could no longer support itself on its left foot.

This experiment repeated on another pigeon, ended in the same way.

I introduced into the pectoral muscles of a pigeon a small bit of wood covered with the oil of tobacco; the pigeon in a few seconds fell insensible.

Two other pigeons, to the muscles of which I applied the oil of tobacco, vomited several times, all that they had eaten.

Two others with empty stomachs, treated as above, made all possible efforts to vomit.

I observed the vomiting to be the most constant effect of this oil, and that the loss of motion in the lower part of the extremity to which it was applied

applied was merely accidental. Not one of the animals to which I applied the tobacco died.

Considerations on the Nerves in Diseases.

Let me be permitted to object for the last time, to the too great readiness in the modern practice of physick, of having recourse to the nerves, to explain the greatest part of the diseases incident to the human body. The ancients had scarcely an idea of this source of so many diseases, to which some of our most modern writers have even believed that the whole of them should without any exception be attributed.

My doubts only relate to the too great extension that has been given to the functions of the nerves, in the diseases of the human body; and I flatter myself that my arguments may make some impression on those who do not suffer themselves to be carried away by hypotheses which have most frequently been adopted, merely because they have never been sufficiently examined.

Hoffman in the third volume of his *Medicina rationalis*, asserts that all the diseases of the human body are nervous: and amongst the more modern writers, Musgrave, a learned English physician, has supported the same opinion. The most moderate of the recent authors who have written on these diseases, have each according to his particular fancy or system

system, either diminished or swelled the catalogue of them, and it is very curious to observe that some take the same pains to exclude several diseases from this number, which others do to prove them to be nervous.

But I must first establish some truths, which will serve to make me better understood in a matter in which there is so much confusion and obscurity. There is no organ in the living body, which may not be disordered by internal and external causes, and afterwards give rise to some disease. Hippocrates and the other physicians of antiquity were well persuaded that if any part of the human frame were disordered, it might disorder another part in consequence; but they did not on that account believe in the *consensus nervorum*, nor in the nervous diseases of the moderns, of which we have made mention in this work. Hippocrates was not ignorant of the power of affections of the mind on man, and how many disorders and changes they are capable of causing in the animal economy; all this I do not to deny, and these are not the nervous diseases now under examination. Besides, the nerves, as every one knows, are the instruments of motion and sensation in animals, and it is on them that the most noble functions, and those the most necessary to life, depend. It cannot then be doubted but that many diseases are nervous, and that the nerves are in many cases the source of very great derangements. But if this cannot be doubted, let me ask physicians what are the certain symptoms by which to distinguish a disease purely

purely nervous; let me ask why it may not be simply a disease caused by the grossest humours, and how they contrive to know that the nerves are immediately attacked in the derangements of the animal economy, which they wish to attribute entirely to them. I do not take upon me the physician, but I have heard several very skilful ones say, that the symptoms of nervous diseases are for the most part equivocal and deceptive.

The moderns have formed a class of motions, and of *sympathetick* diseases, availing themselves of the motions of sneezing and of the iris. We know that the famous anatomist *Meckel*, thought that he explained sneezing, by the shocks sustained by the olfactory nerves, which proceed from the optick nerve, which arises from the *maxillaris superior*, from which is derived another nerve, which in conjunction with a branch of the sixth pair, forms the intercostal. *Meckel*, then, asserts that the shocks sustained by the olfactory nerves, ought necessarily to communicate themselves to the whole of the intercostal nerve, from thence consequently to the phrenick, and to all the muscles of the neck, back, and loins. In fact, true sympathetick motions should be the consequence of mechanical shocks sustained by all the nerves, and by the communication of their organs, and it is thus that the best physiologists have considered it; but these two motions of sneezing and of the iris, are purely voluntary

tary (*a*), not organical, nor sympathetically nervous, and are not produced by external shocks, as anatomists in general have hitherto believed. It will suffice to read on this subject, my work *on the motions of the iris* (*b*), to be persuaded of it. At least I think I have produced both evidence and demonstration on this very obscure subject.

Besides, these pretended nervous sympathies are supported on a principle, the falsity of which has been demonstrated by experiment : if you irritate a nerve, you communicate its motion to the branches it throws out above the part stimulated, and it is on this account that the great Haller, when he became a more skillful anatomist, and a nicer observer, either doubts, or openly denies the pretended nervous sympathies he admitted in his youth.

Undoubtedly no one will say that these motions are nervous and sympathetick, because the mind in producing them, employs the nerves, which are the organs of motion and sensation. This is not the opinion of *Meckel*, nor of those who explain these motions in a way different from mine.

There are physicians who account for all these nervous diseases, by supposing that the nerves are become hard, dry, and tough : others on the contrary, believe them flaccid and relaxed in these cases. “ I have always found,” says the great

(*a*) The sense in which the authour wishes the word *voluntary* to be understood, may be seen in the work cited below.

(*b*) Printed at Lucca.

Boerhaave, “ that however easy it may be to imagine a cause for a disease in explaining it, it is afterwards as difficult to prove that this cause is real, and to be fully persuaded of it.”

Let not those who favour the system of nervous diseases, object that the nerves accelerate and retard the motion of the blood in a thousand cases, as is seen in fear, pleasure, and in so many other conditions. It is true that we cannot deny but that, after these affections of the principle of sensation take place, we observe changes and movements in the living body, which were not observed before ; but this is not sufficient to assure us that these changes are brought about by the nerves alone, and that they act immediately on the blood vessels. The celebrated Haller, completely skilled in anatomy, was of opinion, as is seen in his excellent treatise, “ *De Imperio Nervorum in Arterias*,” that these vessels were bound by nervous rings, with which he found the arteries provided in several places. But as he was also a great experimenter, as well as an excellent anatomist, he soon abandoned this hypothesis, which is contradicted by ocular demonstration. A nerve, in whatever way it is stimulated, is not seen to contract, even with a microscope, and the minutest blood vessels are not observed to shrink or oscillate when irritated by a mechanical stimulus ; neither does anatomy convince us that there are nervous and muscular fibres in these minute vessels, which therefore seem
to

to be destitute of all the instruments of animal motion.

Besides, we frequently see persons violently convulsed, without fever, or any sensible change in the pulse: it is true that the contrary is sometimes observed, but physiologists know that in convulsions, the motion of the blood may be accelerated by the contraction of the muscles that force it from the veins into the heart. The celebrated *Spallanzani* has observed, that the spinal marrow of frogs may be irritated, without accelerating the motion of the blood in the mesenterick vessels of these animals. I have tried this experiment on several other species' of animals, both with cold and warm blood, and have always found the event to be the same; so that the nerves appear to have no immediate action either on the veins or arteries, notwithstanding it is true that the passions of the mind excite very violent disorders in the animal economy.

The derangements I have mentioned, happen, it is true, after certain sensations have taken place in the animal; but this does not prove them to be derived from the nerves, and that the nerves have an immediate action on the organs that are in these cases disordered. It is likewise true, that the followers of Stahl, wishing to establish a general principle which might be applied to all the motions of the living machine, as well in health as in sickness, have had recourse to the mind, as the prime mover of the whole animal economy: I do not

pretend to combat the existence of the nervous diseases in Stahl's hypothesis, according to which all the animal motions are to be regarded as purely nervous, and all the derangements excited in any way whatever in the animal economy, as nervous diseases. Nervous complaints are the immediate consequence of shocks which the nerves may have sustained, or of extraordinary affections excited in the principle of sensation ; and I here principally consider the first class of these diseases. We see indeed that Boerhaave admits of nervous diseases, although he afterwards taxes Stahl's system with falsehood. Haller himself is of the same opinion ; and the two most modern authours who have written on nervous complaints, De la Roche and Tissot, do not hesitate even to reject the more rational system of the learned English physician, Whytt, on the principle of animal motion, and both of them notwithstanding give a greater or lesser support and extension to nervous diseases.

In a word, I do not believe any one will assert, that any motion whatever, any accidental and secondary derangement, is a true nervous disease, because it succeeds to a sensation excited in the mind. In this case it must likewise be said, that the agitations which are caused by fear, pleasure, and grief, are nervous diseases : thus, for example, a laborious and painful respiration, which renders the dilatation of the thorax necessary, will be a nervous disease, notwithstanding there is no affection of the nerves in this case, and that the physician

cian would not certainly seek to apply a remedy to an organ, which was not absolutely injured.

I have pointed out in several parts of this work, that there are poisons, which when applied immediately to the nerves, do not occasion any complaint in a living animal. I have likewise pointed out that these poisons when introduced into the blood, without touching any solid part, suddenly bring on violent convulsions, and the most decided symptoms of what are believed to be nervous affections. Lastly, I have shown, that when the wounds of animals are envenomed with these poisonous substances, the animals are seized with all the symptoms of nervous complaints.

On another hand, I have already made it appear, that convulsions may be excited in animals, without the nervous system being in the least affected, and that the want of an equilibrium in the strength and in the humours, is sufficient to produce the most violent convulsions in the muscles (*a*).

Here are then all the symptoms of nervous diseases, without the nerves having any share in them; and we see at the same time, that these contractions are excited even when the causes of them only seem to act on the humours of the animal, and whilst these causes are found innocent and ineffectual to the nerves, in whatever way they are applied to them. It is not sufficient then to see

(*a*) See the first part of my work on the motions of the iris, quoted above.

all these symptoms, to enable us to pronounce with certainty, that the disease is purely nervous.

But there is an argument which seems to leave no refuge to the most prejudiced and most obstinate skeptick. It is observed in regard to the motions, as well involuntary as spontaneous, which are wrought by the-nerves, that if you stimulate the nerves which are carried to the organs of these motions, the latter will constantly and necessarily follow. This is a general law for all the muscles and nerves, in all animals, and admits of no exception. It becomes then a certain law, a sure principle, and an infallible criterion of the nature of these motions.

The heart, of all others, is the organ first affected in the passions of the mind and in nervous affections; and it is on this first change, that a number of others which accompany it depends. Let the breast of an animal with cold blood be opened, (this experiment is subject to less uncertainty in these animals than in those with warm blood, on whom the effect is however the same,) and let the nerves that are sent to the heart be stimulated, in any way whatever; it will not on this account quicken its contractions if it is in motion, nor will recover its motions if it is at rest, although it is still in a state of contracting on the least stimulus offered to its fibres. It is in vain that long pins are introduced into the vertebral canal, and that the spinal marrow and brain are mangled, the heart continues still insensible. The nerves
then

then which are carried to the heart, are in no way the organs of motion in this muscle, although they are certainly so in all the others. They can therefore never cause any sensible change, however the animal may be affected. The experiment is certain, and the consequences of it direct (*a*), and it would be a downright contradiction, that the motions of the heart should be brought about by the nerves, whilst the latter are not capable of exciting these motions, as has been experimentally demonstrated.

It cannot then be advanced with certainty that the changes in the heart which usually accompany the affections of the mind, are wrought by the immediate channel of the nerves, and not otherwise; and the only inference which an unbiassed philosopher can draw from all that we have said is, that we are ignorant through what medium, and by what mechanism, the affections of the mind influence the heart.

It may perhaps be said that the *sensitive principle* in the animal may make certain impressions on the nerves, which mechanical stimuli cannot imitate; but this supposition is contradicted by daily experience, since the least stimulus to a very small nerve at the extremity of a muscle, is sufficient to give it motion: this is a demonstrated truth which has never been contradicted by observation.

(*a*) This important truth has been demonstrated by the author in his treatise on Animal Physicks, Volume. 1 st. p. 92. published in Italian in Florence at 1775.

It may be opposed that the experiment of the immobility of the heart to the shocks that are given to the nerves, is contradicted by several of the most famous naturalists: there is no other reply to this than the recurring to experiment. Any one in doubt, may easily satisfy himself by taking a frog and, after opening its breast and cutting off its head, wait till the heart is still, or moves but slowly, to render the experiment more decisive, and then thrust a pin deep into the spinal marrow: he will soon see the event. Care must be taken not to leave the frog to itself, but to secure the feet well, otherwise there will be a risk, amidst the violent convulsions that will be excited in all the other muscles, that the heart itself may be agitated, and caused to move in quite another way than by the action of the nerves. This is undoubtedly what has deceived so many good anatomists, who have believed that this motion was occasioned immediately by the nerves. I refer my readers to the experiments related on this subject, in the work cited above.

It is in my opinion then, a matter demonstrated by the fullest evidence, that no motion of the heart can be in any case brought about by the medium of the nerves, although the heart is, of all the muscular organs, the most susceptible to the affections of the mind. Some grounds may after this be permitted me for doubting that whilst the motions of the heart are never influenced by the nerves, those of the other muscles are invariably caused by their immediate action.

A thou-

A thousand like arguments may be brought in favour of the hypothesis of the nerves; but they are all indirect, and only prove that a sensation in a living animal, is followed by a motion in its body: this is not yet sufficient to demonstrate that the effect has been immediately produced by the action of the nerves. Fear retards or quickens the motion of the heart; and yet, as has been seen, the nerves have no immediate action on that organ, notwithstanding it be true that this change is the consequence of a sensation.

The learned translator in French, (*a*) of the new edition of Whytt's English publication on nervous diseases, introduced in the 151st page of the first volume, a note against his author, which seems to me so very apposite to the present subject, that I give it entire. Whytt had maintained that hysterical diseases, (or rather their symptoms) and the hypochondriacal diseases of men, were in effect the same; and that both of them were simply nervous. The translator observes in his note that "this is a sure method to confound and perplex." He adds that "the symptoms Whytt has related, are effects which in a greater or less degree are common to all diseases. There is not any one in which the nerves are not affected; but the business of a physician consists in knowing, as well as he can, the cause of the disease. It is certain then, that three fourths of the diseases of women have their origin in the matrix. The ancients were therefore

(*a*) The translation was printed at Paris in 1777.

“ not mistaken when they bestowed the epithet of
 “ hysterical, on those diseases in women in which
 “ the nerves display the greatest number of symp-
 “ toms.”

Several substances are announced in the *Materia Medica*, as *nervines*, because they are supposed to have a benign influence on the nerves. For my part, I believe it would be very difficult to prove clearly, that a remedy has an immediate action on the nerves, and not on the other parts of the body. I do not mean however to speak here, of certain substances which, being capable of dissolving, corroding, and gnawing the nerves, have assuredly a true immediate action on them. It is true, that spirituous substances applied to the nostrils, seem to act instantly on the nerves, and in a very different way ; but it must be considered, that they not only cause a simple mechanical percussion on the pituitous membrane, but excite a particular sensation in the animal organization. The natural light of the sun, in whatever way it is applied to the body, is quite innocent, and is not felt by it ; but meeting in sudden contact with the eyes, may make such an impression on them, as to cause a violent pain, and even tears. The eye alone is susceptible of the impressions of light, and the other parts, although endowed with sensation and life, are insensible to it. The difficulty then I have just mentioned, only proves that a stronger percussion on a determined organ, excites a stronger and quicker sensation in the animal machine, than a weaker

weaker one, which is altogether natural, and argues nothing in favour of the hypothesis of the nerves.

But who can assure us that the particles of scents may not penetrate, even in a few instants, through a quite porous body, filled with canals and fluids incessantly in motion?

I know it is commonly believed that opium produces effects, when introduced into the stomach, which are not observed when it is applied to the other parts of the body. But who again will here assure us, supposing it an incontestible fact, that certain juices which are only found in the stomach, are not requisite to detach the most active parts of this substance, and that this viscus does not contain very fine vessels or porosities, proper to receive them, which are not found elsewhere? I am not ignorant of its having been asserted, that opium, when it is immediately applied to the nerves, produces a palsy of the muscles: but I recollect to have seen some years ago, a disorder of this kind clearly occasioned by the spirit of wine in which I dissolved the opium, since it did not ensue when I tried it dissolved in water. This circumstance appears to me so interesting, that I shall not fail to repeat the experiment at my leisure, particularly as the authority of Monro, who has found the contrary, may be opposed to mine:

There are in the living body passages we are yet ignorant of, unknown powers, and latent principles. We see the necessity of admitting them; but we are ignorant of their nature and mecha-

nism. If it is a misfortune to be in the dark as to a truth, it is a still greater one to subscribe to an error. We do not attempt to draw erroneous consequences from things we are ignorant of; but a mistake necessarily leads us into many others. It is much better then to be ignorant of a truth, than to admit an error.

I have frequently, in many parts of this work, spoken of the influence of the nerves in diseases. I have said too little on this subject, in proportion to its importance, and certainly a great deal too much in a work that has quite another object in view; but I could not resist the evidence my experiments held out to me. They have led me, in spite of myself, to make some applications to certain phenomena in the animal economy.

I do not pretend here to undeceive those who are prejudiced in favour of a principle which supplies all the wants of a system of medicine, with a readiness proportioned to the uncertainty and obscurity of that system; and I am not to learn that it is a great help to those who profess it. The character of obscure and indeterminate hypotheses is, that they adapt themselves to every thing, because they are capable of being modified according to particular cases; but this is exactly what ought to make them suspected.

I ought however to make an exception here in favour of several very skilful physicians, who have frankly acknowledged that my experiments render the nature of nervous diseases in general very suspected.

pected. I shall select from the names of many others, that of the celebrated English physician Sir Robert Pringle, who told me that he had never too great a belief in nervous diseases, and that for the future he should have less faith in them than ever.

I repeat that I will not deny, as the great Albinus has done, but that the nerves may in general be the organs of sensation, or of motion, in animals: this would be going too far; but it may be doubted that all the motions we observe in animals depend immediately on the nerves, and that the substances which have excited them, as has been pretended of the venom of the viper, the *ticunas*, and the cherry-laurel (to oppose the action of which all my reflections ultimately tend) have immediately acted on the nervous system. Neither can it be denied, but that when the *principle of sensation* is affected, a thousand disorders in the animal economy ensue, any more than it can be affirmed that the nerves can be acted on with impunity; but it does not yet follow from this, that all the diseases commonly ascribed to the nerves, are owing to the nerves alone; that they may not rather depend on the humours; that both medicines and poisons act immediately on the nerves; and lastly, that the nerves (it is to this that I principally confine myself) have an immediate action on the other solids.

Irritability seems independent of sensation in an animal; and there is besides nothing which demonstrates

monstrates that the muscles are only capable of moving by the action of the nerves. The principle of sensation and the nerves, may have agreements with the blood and other humours, which we are yet ignorant of, and these humours, more or less changed, may exert their influence against the solid parts of an animal. It is permitted to suppose every thing, rather than to resist a direct and luminous experiment. It is permitted, when we are about to establish a truth, to imagine a new construction of parts and of organs, and to furnish new agreements. But to invent a new construction, and to admit unknown agreements, in supporting an hypothesis, is building the enchanted palaces of Ariosto, for the reception of Roger and Alcine:

E X P E R I M E N T S

MADE AT LONDON IN 1778 AND 1779.

On the Reproduction of the Nerves.

THE knowledge I had acquired of the true structure of the nerves, and of the primitive cylinders of which these organs are formed, as will be seen in the following treatise, made me desirous of applying this knowledge to the animal economy. During my stay in London, I did not fail to examine the museum of the celebrated Dr. Hunter. Mr. Cruikshanks, a young man very promising in the science of anatomy, and dissector to the Doctor, there showed me a glass, in which he told me was preserved a reproduction of a dog's nerve, one of the eighth pair, which he had cut. The circumstance appeared to me altogether new, and deserving the utmost attention.

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He added, that the nerve had been cut in the living animal, and that he had removed a portion about an inch in length; and indeed, for the space of about an inch, it appeared very different from what it was in all the other parts. It was much enlarged, quite irregular and uneven, and seemed formed of a different substance from the rest.

On seeing this preparation of Mr. Cruikshanks', I had two reasons for doubting the truth of the fact. One was, that I had never observed in any one of the experiments I made at Paris on the venom of the viper, a true reunion of parts in the sciatick nerves, which I had so often cut. The other, that in the nerve in question, there might very well be a reunion of one part to the other, but not a true reproduction of the two extremities, so as to form a single nerve as it was before.

These suspicions made me desirous of a particular conversation with Mr. Cruikshanks, during which I asked him, amongst other things, what was Dr. Hunter's opinion on the subject. He told me ingenuously, that the Doctor did not perceive a real reproduction of the nerve in these experiments, and suspected very strongly, from the great difference betwixt the external structure of the part cut, and that of the other parts, that it was not the case. I then understood from Mr. Cruikshanks, that he had at different times, in the space of eighteen or twenty days, divided the two nerves
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of the eighth pair, and the two intercostal ones, in the same animal, and that they all seemed to be equally reproduced.

It is beyond a doubt, that the cut extremities of the eighth pair, and of the intercostal nerves, are capable of reuniting, although a part of them be taken away, and Mr. Cruikshanks' excellent experiments demonstrate it in such a way, that it cannot be doubted a moment; but it is not yet certain that these nerves return to their prior state, by forming a continuance of a true nervous and medullary substance, and continue to perform their usual functions. This absolutely remains to be proved. It is true, that a continuance of life in the animal, after the nerves have been cut, as well as there being no sensible change in the operations of the heart, leads one to suspect that the eighth pair of nerves has been truly and full reintegrated; but as it has not been yet proved that the nerves are absolutely necessary to the motion of the heart, and as it is known that this viscus receives nerves from other parts, it is to be doubted, whether this ought to be regarded as a true renewal of the nerves, or should be only deemed a simple union of parts, brought about by the means and interposition of an heterogeneous substance, composed of the cellular membrane. My observations on the structure of the nerves enabled me to ascertain with certainty, whether they are really reproduced or not, and this has engaged me to make several experiments on the subject. I preferred rabbits,

as the most convenient for such trials, and easily to be procured. I destined a great number for the cutting out the sciatick and crural nerves, many others for that of the eighth pair, and some for that of the intercostal and eighth pair together.

In six rabbits, I simply cut the right sciatick nerve; and removed a portion of about six or eight lines in length, in six others. Some of them lived eighteen or twenty days, and others died in the space of four or six. Others lived so as to enable me to examine the nerves I had cut, at the end of thirty days or more.

I could not perceive the smallest appearance in any of these animals, of a nervous reproduction. In all of them the extremities were as smooth and even as when I first cut them. The nerves were in every part white, and neither thickened nor uneven. In a word, I had the fullest assurances that in the animals I had employed, there was no reproduction of the nerves.

I must observe here, that I might easily have been deceived in two particular cases, had it not been for the knowledge I had acquired of the structure of the nerves and muscles. In one of these I had simply cut the sciatick nerve, in the other I had removed a portion of it of about six lines in length. In both I could not discover the two extremities of the nerves, and I found them perfectly covered and bound together with a substance partly cellular and partly fleshy. What was very
singular,

singular, the more I removed this substance with a scalpel, the more the union and reproduction of the nerves seemed to be really brought about. But the microscope soon freed me from this suspicion, and I at length found that this substance was not formed of the primitive nervous cylinders of which I shall speak in the ensuing treatise, but of a cellular membrane, and of primitive fleshy cylinders.

The observation of these latter particulars made me suspect that the intercostal nerve and that of the eighth pair, had only presented an apparent reproduction, because in all the cases where I had cut the sciatick and crural nerves, there was not the smallest appearance of a reunion or reproduction of the parts.

'Tis true that the constant restlessness of the animal may prevent a reunion of the divided nerves in these cases; but the two extremities of them should at least appear a little changed and rounded, as happens in all the parts where there is a reproduction or reunion, after their being cut.

But it still belongs to experiment to decide; and we must not form conjectures where that can be recurred to.

I cut one of the eighth pair of nerves in a dozen rabbits, and in a dozen others removed a portion of it of six to eight lines and more in length; in these last I also removed an equal portion of the intercostal nerve. One of the first died in four days, two of the second in three, and a third in eight. Those
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which survived, had no perceptible complaints, and ate as usual a short time after the operation.

In some of these rabbits, I cut at the end of twenty-five days the other nerve of the eighth pair, and in some others removed a portion, as well of the nerve of the eighth pair as of the intercostal. Of six of these last, three died in a few days.

This is not the place to give a detail of all I observed in these animals; I shall for the present content myself with relating a few general observations.

In two of them I could observe no change in the divided nerves, although I examined one at the end of eighteen days, and the other at the end of twenty-seven. In a third I observed that the cut extremities of the nerve had changed their shape and colour, but there was no true reunion, nor apparent nervous reproduction.

In a fourth, which died twenty-three days after the operation, the extremities of the nerves were a little elongated in a conical shape, but were not united: there was indeed a flat membrane betwixt the ends of the nerves, which united them imperfectly. In all the others where the nerves had been simply cut, the parts seemed to be united, had changed their colour, and were thickened. They were in general covered with a cellular membrane, enlarged, and a little red.

As to the nerves of which I had removed a part, there was a reunion which likewise seemed to be
caused

caused by a cellular substance much swelled, unequal, and full of blood vessels. The cut extremities of the nerves were whiter than in any other part.

I sought for the spiral form of the nerves in these reproduced and unequal parts, and although I thought I distinguished in a greater or less degree, in more than one, the white spires or bands (*a*), I could not however distinguish them from one extremity of the nerve to the other, so that I was uncertain whether the part reproduced was not rather cellular than nervous; neither could I assure myself by cutting the cellular membrane, a part of which I likewise removed, whether the primitive nervous cylinders really passed from one part of the nerve to the other, although I saw them extended through this reproduced cellular membrane. I confess that I had not all the conveniences necessary to render me certain in so important an enquiry, and which I found at the same time a very difficult one.

All that I can say with certainty is, that the cut ends of the nerves are prolonged, that they alter their shape and colour, and that they are united by a substance betwixt them, which is a prolongation of the cellular membrane itself belonging to the two cut parts of the nerves. The winding cylinders and blood vessels pass from one part to the other, and

(*a*) It will be seen in the following treatise, what this spiral shape or these white bands are, of which mention is here made.

there is an union of the whole, as if the cellular coat of the nerves, which is much thicker and more unequal than in the rest of the nerve, was quite entire.

The difficulty of establishing by immediate and ocular demonstration, whether in the above cases the nerves are really reproduced, or are simply fastened together by a cellular membrane, made me redouble my attention, and multiply my experiments.

I do not think I am wrong in advancing at this time, as a certainty, that a like reproduction of the nerves may take place; although it cannot be always demonstratively proved, and perhaps is not always brought about, although the nerves seem to be reunited, and reproduced.

I can say to a certainty that I observed it in two particular cases, and in one of the two in so evident and sure a way, that I think it proper to give here the figure and description of it. But I repeat again, that a simple continuity of parts betwixt the cut ends of a nerve are not sufficient to determine whether the nerves are really reproduced, and that it is moreover not sufficient that a cellular substance is produced and elongated, although it be a continuation of that of the nerves themselves. We must be certain that the nervous cylinders pass without interruption from one part to the other.

PLATE VII. FIG 3. represents a nerve of the eighth pair belonging to a middle sized rabbit; I viewed it with a lens which magnified about three
times

times in diameter. The part of this nerve I had removed, was about six lines in length; and I dissected the animal twenty-nine days after the operation. I found a reunion of the two cut extremities of the nerve, but that the nerve was smaller than elsewhere at the part where the reunion took place, which I have marked *r, r*.

At some distance from the point *r, r*, precisely where the nerve was cut, were seen two white spots *nn, nn*, as they are represented in Fig 3 and 4. These two spots formed two opaque rings round the nerve, and close to these rings the nerve began to decrease on each side in a conical shape, and to prolong itself in this way as far as *r, r*, where both cones met. In Fig. 3, and still better in Fig. 4, are seen the spiral bands of the nerve, and these bands were continued as far as *r, r*, where they were not so distinctly seen.

At the two spots *nn, nn*, the bands seemed to be interrupted, or rather, the white colour of the nerve in this place prevented their being seen.

The nerve was smooth throughout, and was likewise so all over the two nervous cones. I wished to see the nerve through a strong lens, and to examine the cellular membrane. Fig 5, represents the nerve observed with a very strong lens. I found it covered with the usual cellular membrane. I then examined it with the strongest glasses, and as is seen in Fig. 6, found it formed of primitive nervous cylinders, of which as will be seen in the ensuing treatise, every nerve is composed. These cylinders diminished in diameter as they approached each other to the point *r, r*, of

the two cones, and they were plainly distinguished to be continued, and to pass from one side to the other. Fig. 7, represents the same nerve, but partly torn by needles, to show more distinctly the continuity of the primitive nervous cylinders.

Two things concur to persuade me that a true reproduction of the nerve is brought about. One is, the appearance of the spiral bands, which are found even in the renewed and smallest part of the nerve; the other is, the continuity of the primitive nervous cylinders, which removes the smallest suspicion of doubt.

I have had another instance of a reproduction, almost exactly similar to that I have described. The two white spots were likewise seen here, and at the part cut, the two cones which met at their points. The spiral bands were continued in the cones, and in the continuity of the primitive nervous cylinders was distinguished in every part of the nerve.

It is an established truth then, that the nerves of the eighth pair are capable of a reunion, not only when they have been cut, but likewise when a portion of them, several lines in length, has been removed. In the first case, there is a true reunion of parts, a real continuity of substance, and in a word, a complete continuance of the primitive nervous cylinders and of the external coats which enclose them. In the second case the nerve is reproduced, that is to say, its nervous substance is increased at the two extremities, and in the prolongation of it, these extremities meet, so as to form an homogeneous, continued, and uniform whole.

It is singular that the two extremities of the divided nerve, meet so exactly as to be capable of uniting together; particularly when a very large portion of it, an inch for example, has been removed. In these cases, it seems altogether improbable that the cut parts should meet so well, and this junction is rendered still more difficult by the great derangement in the situation of the nerves, that attends the operation. But we must in the first place consider, that all the parts of the neck, the muscles particularly, continuing their accustomed functions, oblige the nerves to regain that situation which these parts and these motions require.

Again, I must here observe, that having twice expressly changed the direction of the cut extremities, so that they presented themselves contrary ways, I did not find in the sequel that these parts were reunited, or had met each other.

Had I had more time, I should have endeavoured to determine whether this faculty of reproducing, in the nerves of the eighth pair, and the intercostal ones, be common to many other nerves; which seems probable. The sciatick nerves, which are probably of the small number of those that have not the advantage of reproducing, are perhaps prevented from it, from there being too much motion in the parts where they are situated, and it would in all likelihood ensue if that motion were diminished. It may likewise be a property only belonging to those nerves that are the most essen-

tial to life ; but all these points may easily be cleared up by immediate experiments.

Every one must now see, that a great many interesting truths to the practices of physick and surgery result from what I have just related. We may now conceive how sensation and motion itself have returned to certain parts which were almost entirely detached from the bodies of animals. The nerves were in these cases reunited, and continued to be the instruments of motion and sensation. In many cases of urgent necessity, there will be less dread of cutting some particular nerve, in doing which it will be only necessary to take care that the cut extremities be placed opposite to each other.

There is a physiological experiment to prove the reunion of the divided nerves, which want of time has prevented my making. After cutting the phrenick nerves, if once a reunion of the divided parts takes place perfectly, and there is a true continuity of the nervous substance, the diaphragm should contract itself, on irritating these nerves, in the part towards the head, above where they were cut.

OBSERVATIONS
ON THE
PRIMITIVE STRUCTURE
OF THE
ANIMAL BODY.

TO WHICH ARE ADDED,
REMARKS ON VEGETABLES AND FOSSILS.

*Observations on the Structure of the Nerves, made at
London in 1779.*

OF all the organical parts of which a living animal is formed, there is no one in my opinion, the structure of which is less known, and the knowledge of which at the same time is more im-

portant than that of the brain, and the nerves that are derived from it.

The best authours have advanced nothing but mere hypotheses on these parts, and the most penetrating naturalists have made observations which have been contradicted by other naturalists equally skilful; so that after an unprejudiced examination of the whole, we are forced to confess the having learned nothing, and that the texture of these organs is obscure and uncertain.

The celebrated Haller, after comparing the different opinions of anatomists as to the structure of the nerves, and principally examining Lewenhoeck's observations on the structure of these parts, ingenuously confesses that nothing but simple conjectures can be advanced thereupon. He however is disposed to believe that the structure of the nerves may be cellular.

Among the latter naturalists who have examined the animal body, there are two particularly, who deserve our notice, the learned Father della Torre, known by several philosophical works, and still more by the microscopical observations he has at different times published, and M. Prochaska, a skilful professor of anatomy at Prague, who has given us two very interesting microscopical works, one on the fleshy fibres, the other on the structure of the nerves.

Father della Torre (*a*) examines the two substances, cortical and medullary, of the cerebrum

(*a*) *Nuove Osservazioni Microscopiche.* Napoli, 1776.

and

and cerebellum ; he likewise examines the medulla oblongata, medulla spinalis, and lastly the medullary substance of the nerves. He finds that all these organs are nothing more than a mass of numberless globules, transparent, and swimming in a diaphanous fluid. These globules, he says, are very small in the medullary substance of the nerves, in which they are placed in almost a right line, so that they seem to compose simple threads and fibres, instead of which in the cerebrum they are very large, less in the cerebellum, and still less in the medulla oblongata and medulla spinalis, where they are not placed in a right line, but mixed confusedly together.

Prochaska (*a*) does not admit of any difference betwixt the cortical and medullary substances of the brain, finding both of them to be formed of an immense quantity of globules united together by an elastick and very transparant cellular membrane. He does not agree with Father della Torre as to the difference in size of these globules, but observes with him, that they are disposed in the nerves in a right line, and appear like a longitudinal fibrous structure.

Albinus, who joined the use of the microscope to that of the most subtil injections, denies that the cortical and medullary substances of the brain are purely vascular. Others have not only considered the substance of the brain, but likewise that of the nerves, as a nonorganick substance, and a mucous

(*a*) *Structura Nervorum*. Vindoborn, 1779.

pulp. Others again have taken it for a substance purely cellular. And all of them have thought the use of the microscope necessary to assist the naked sight.

By the little that has been said, it is easy to perceive how great an uncertainty we are in, not only in regard to the structure of the nerves, but that of the brain itself. The observations of Father della Torre and M. Prochaska however deserve our consideration. Independent of their skill in observation, they were not ignorant of all that other naturalists had seen, or had believed to see, before them. They were therefore less likely to be deceived, in running over a beaten path, and so much the more deserve our utmost consideration, as entirely agreeing in regard to the primitive structure of these parts, which both believe to be formed of simple globules.

Being in London in 1779, I heard that the celebrated anatomist, Mr. Monro of Edinburgh, had made some important discoveries in relation to the structure of the nerves ; but as I was ignorant not only of the detail, but likewise the consequences of his observations, I addressed him in the following manner :

“ S I R,

“ Although I have not the honour to be personally known to you, I take the liberty to request
“ some

“ some information as to the interesting discoveries
“ in anatomy, which I am told you have made on
“ the structure of the nerves. I am informed you
“ have published something relative thereto in a
“ journal, and in two papers read to the Medical
“ Society at Edinburgh. As I am busied on this
“ subject, I shall be glad to know how far you
“ have carried your researches, that I may be en-
“ abled to render you all the justice you deserve,
“ provided I determine hereafter to give my ob-
“ servations to the publick. I regard your dis-
“ coveries as already published, and consequently
“ antierior to mine, but should I write on this sub-
“ ject before I am fully acquainted with them, it
“ would be out of my power to attribute to you
“ all that is your due, and by passing over your
“ works in silence, I should incur the suspicion of
“ wishing to appropriate to myself the discoveries
“ of others. You can run no risk in communi-
“ cating to me what you have done, since on one
“ hand, you have already made your discoveries
“ known to a publick body, and on the other, my
“ letter will be always a security to you, against
“ any improper use I might make of your com-
“ plaisance. Men of real merit are rarely suspi-
“ cious and shy : this induces me to hope that you
“ will bestow some favour on one who renders you
“ the greatest justice, and who desires to be in-
“ structed by the discoveries with which you have
“ enriched and advanced the science of anato-
“ my.”

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Having received no reply from Mr. Monro, and fearing that my letter had not reached him, I conveyed a copy of it by his pupil Mr. Crawford (*a*), who was then in London, begging him to have it delivered by some trusty person into Mr. Monro's own hands: it was quite ineffectual, as I had no reply from this celebrated professor of Edinburgh.

I heard in the mean time, that mention was made of Mr. Monro's discoveries in the first part of the sixth volume of a journal, entitled, Medical and Philosophical Commentaries by a Society in Edinburgh, printed in London in 1779. Not being able to obtain any information on the subject from Mr. Monro himself, as I had flattered myself, and should have wished, to do him every justice, I am under the necessity of transcribing from the commentaries I have mentioned, the article relative to the professor's discoveries, which, to be more certain, I do at full length. “ Dr. Alexander Monro, professor of anatomy at Edinburgh, “ has lately taught in his lectures many particulars “ respecting the brain and nerves, which are “ tirely new, and which must lead to very different “ opinions respecting these organs, from what any “ physiologists have hitherto entertained: he has “ also read a paper on the same subject in the philosophical society of Edinburgh.

(*a*) Authour of an excellent publication on occult heat.

“ Of his descriptions, which are founded entirely on microscopical observations, conjoined with nice dissection, and which are illustrated by numerous engravings, we cannot at present pretend to give a sufficient account. We may only observe, that he finds the structure of these parts to be very different from what was formerly imagined. He has discovered that the brain and nerves, in all classes of animals, in place of straight fibres, are every where composed of convoluted fibres, nearly $\frac{1}{9000}$ parts of an inch in diameter, which do not seem to be hollow, but solid.

“ He finds that their extent in the system is much greater than has even been believed; and that they not only enter the composition of parts intended for sense and motion, but also of every other part of the body. Thus he has discovered them penetrating to the very extremities of the longest hairs; and in great numbers entering the composition of the cuticle and nails. He farther alledges, that the bulk of all our organs depends chiefly on their nerves; and that when muscles or bowels are cut transversely, many more nerves are divided than when the same operation is done upon the cord, called by anatomists the nerve of that part.”

“ He finds also, that a system of convoluted fibres, in every respect analogous to the nerves of the human body, is to be discovered throughout the whole vegetable kingdom. Nay, that the
“ metals,

“ metals, semi-metals, earths, and salts, consist al-
 “ most entirely of convoluted and serpentine fibres,
 “ similar to the nerves of animals in size and
 “ shape.

“ How far the testimony of future observers will
 “ confirm Dr. Monro's descriptions, is not for us to
 “ determine. But we may venture without hesita-
 “ tion to assert, that if the account which he has
 “ given stands the test of sceptical scrutiny, it must
 “ be considered as the greatest anatomical discovery
 “ which has been made for many years.”

Mr. Monro's discovery seems to me principally to consist in his having found that the brain and nerves are composed of convoluted, and not of straight fibres; that these fibres are about the $\frac{1}{9000}$ part of an inch in diameter, and that they are not hollow, but solid. He adds, that these fibres not only enter into the composition of the organs of sensation and motion, but likewise into that of all the other parts of the body; and he likewise finds them in hairs, in the epidermis, and in the nails.

He will likewise have it, that the principal mass of all organized parts is composed of these convoluted fibres, that is to say, of nerves:

Lastly, he finds a system of the like convoluted fibres, in all the nerves of the human body, and in the vegetable kingdom; and believes that fossils are almost wholly composed of convoluted fibres, similar in size and form to the nerves of an animal.

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We may certainly conclude from all this, that Mr. Monro regards the convoluted fibres in animals, as of a nervous nature, although he agrees besides, that they are not found to be instruments of motion and sensation in all bodies, as they certainly are not in fossils or plants, any more than in the nails and hairs.

Although the greater part of these discoveries of Mr. Monro seems paradoxical, this is no reason for denying them ; and the authority of the professor would alone be sufficient to induce me to examine his observations very attentively, even if the importance of the subject, which is in itself very interesting, did not require it.

The new discoveries of Mr. Monro, are altogether different from the observations on the structure of the brain and nerves, made by those who have preceded him. I thought it proper then to examine this matter as if it had been entirely new to me ; and the opinions of writers have only served to make me more cautious in pronouncing, even where my observations were the most constant.

I wished to examine the nerves as they appear to the eye, in a living animal ; without touching the parts that compose them. I separated alone, the parts altogether of a different nature, that were contiguous to them. It was not difficult for me to perceive that they appeared formed of bands more or less regular, or of alternate white and dark spots.

PLATE III, FIG. 1. Represents one of these nerves in which the bands were more regular and distinct than in the others. On examining them with a lens which magnified six times, every thing was seen better and more distinctly. I detached the nerve from the animal, without pulling it in any way, and examined it on a glass. The bands appeared exceedingly regular, they were all equally large, and the spaces from one band to another, were equal to each other, and equal to the bands themselves. I thought at first, that these bands formed a true spire in the nerve, or rather, that they turned spirally, as a ribbon would do round a cylinder. This idea seemed to agree with what I observed, and what seemed still more to confirm it, the nerve rolling in this way on itself, the bands seemed to continue their circumvolutions throughout, and I could not perceive them to be formed of detached rings, placed at equal distances.

I wished to see whether this singular structure, or spiral appearance, were common to all the nerves, and spared neither time nor labour to assure myself of it. In the great number of nerves of animals I have examined till this time, I have seen very few in which the bands were as regular as in the above cited Fig. 1.

These bands in general seem to form different angles, and to cross each other, and are often seen of different sizes.

But whether great or small, regular or irregular, whether they cross each other, or proceed in

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a parallel direction, they are observed in all the nerves, to the very brain and spiral marrow, that is to say, to the part where the nerves form themselves into threads, or cylinders. A degree of attention is required to observe the spires in many of the nerves, above all at the place of their origin.

When they are too abundantly covered with cellular membrane, or if it be in flakes, it must be removed to better distinguish the spires. In many of the nerves they are seen with the naked eye, without any need of preparation, so that this appearance of bands, is a constant and certain characteristick in the nerves; and these organs of motion and sensation appear to be composed in a great part of these white bands, since when simple and regular, they occupy about half the length of the nerve.

These nervous bands are not destroyed, although they are not so distinctly seen, when the nerves themselves are strongly pulled out, provided the distension be not extremely great: in the latter case they may be so changed as not to be distinguishable. The tenacity in the nerves, to preserve this appearance of bands, seems to confirm still more the latter being true spires, and that they roll round the nerve, as a ribbon does round a cylinder.

I could not however conceive how the anatomists who have searched into the nature and composition of the nerves, and still more the microscopical observers, could escape the seeing these

bands, which are so readily and constantly observed in all of them. I have at least found no one who speaks of them, although some one whose works I have not read, may have noticed them: this is of but little consequence; but it is very essential for us to know that the nerves present themselves in this form, a circumstance which may tend very much to a knowledge of the nature of these organs, that are so important to animal life.

Let us pass to the examination of the different appearances of these bands, and see with how many variations the nerves present themselves, to the attentive eye of an observer.

Plate III. Fig. 1. Represents a nerve magnified about six times, with a lens. *cc, cc, cc, cc,* are white bands, all equally large, and at equal distances. *oo, oo, oo, oo,* are the opaque parts of the nerve, equal in every respect to the bands.

Fig. 3. of the same plate, represents a nerve magnified eight times by a lens. The bands in this one cross each other at different angles, and in different parts of the nerve. Fig. 2. is another nerve observed in the same way. The bands are more distinct, and approach each other in some places, instead of crossing, but without any regularity. The nerve in Fig. 6. likewise shows the white bands, some of which unite, and others cross. The little regularity in the bands led me to suspect that there were several orders of them in the same nerve, and that they perhaps took different directions. Fig. 7. almost fully confirmed me in this belief

belief. The bands crossing each other are there seen in the midst of the breadth of the nerve, forming both sharp and obtuse angles, perfectly equal to each other. But this hypothesis, or this suspicion, was not yet confirmed by observation, and might very likely be false. In multiplying my observations, I found in several of the nerves a double order of these bands, which met each other, as the cogs of two wheels do, that are fixed to each other.

Fig. 5. represents this double order of bands very perfectly, as I observed them in a nerve, with the assistance of a lens which magnified six times. The bands of the two orders *a r, o c*, were equally large, and every where at equal distances, and ran one into the other for more than a third of their length; as the band *o* is seen to enter into the band *a*, and likewise the band *c* into the band *r*. This new observation convinced me still more that these bands ran along the nerve in the form of concentrated spires, equally distant, and every where of an equal diameter. It is true that I regarded them as formed of two nerves united together by a common cellular sheath, and could in this way account for all the irregularities I observed in them. The nerve in Fig. 8. completed this persuasion. *r, a, r, a*, point out one of the two nerves, and *a, o, a, o*, the other. The line of separation betwixt the upper and lower bands is distinctly seen, and this line *a a*, can be nothing else than the union of the two nerves. I found it no longer difficult to assure myself of the truth of this supposition, all that

was required being to strip this nerve of the common coverings, and afterwards to separate the nerves of which it might be composed. Fig. 4, represents in effect, not only a separation of the nerve from the common sheath, but likewise of the nerves that compose it. There are four of these nerves, viz. *ab*, *ce*, *or*, *fm*, and in each of them the bands are simple, without meeting each other, or forming angles. It must not however be thought that a nerve which presents a single order of bands is perfectly simple, that is to say that it may not be formed of other smaller ones; all my observations prove the fallacy of this. I have always found that the largest nerves are formed of smaller ones, and these again of smaller still; and in the last, which I shall call simple, the bands always appear regular and without forming angles.

The nerves then are small as hairs, and perhaps still smaller; notwithstanding which the bands are distinctly seen, even with the weakest lens'. When they are larger, and form other nerves, the cellular membrane unites them and wraps them up in such a way, that the eye cannot penetrate far into these substances, and consequently a single order only of bands is distinguished.

This wonderful structure of bands is common then to all the nerves, even the smallest of them, and the irregularity of these bands is only caused by the bands of the smaller nerves, of which a large one is composed. I have endeavoured to strip them of their sheaths, as well particular as common,

with the point of a sharp needle, so as not sensibly to change their structure, and am fully convinced that this operation does not destroy the bands, which therefore seem clearly to depend on the primitive structure of these organs.

Persuaded lastly that these bands were not an appearance or opticle illusion, and seeing that the very irregularities of them confirmed me still more in the opinion that they were so many spires, that is to say, that they turned uninterruptedly round the nerve from one extremity to the other, I proceeded to a search into the nature and composition of them.

The white colour may at first sight induce one to judge that it is a pure medullary substance; but this can alone be decided by observation.

My first attention was to examine a small nerve covered with its own natural cellular membrane. Fig. 10. represents it in the way I saw it with a very strong lens, and covered with water. The two extremities of the nerve, *a, a*, were a little transparent, and seemed formed of very fine threads, interspersed with a great number of very large oval globules. These oval globules and small threads are common to all the sheaths of nerves. The opaque and middle part of the nerve *a, a*, seemed formed of parallel winding threads, as is seen in the same figure. *m, m, m*, are oviform globules observed in the cellular membrane of the nerve; and *r, r, r*, are the filaments of the cellular membrane itself, swimming in the water.

Every time I examined the nerve in this manner, that is to say, with a very strong lens, the bands were no longer visible, and I could not account clearly for this disappearance.

Instead of spiral bands observed before, I saw parallelwinding fibres, every where of an equal thickness, running all along the nerve; and yet when I examined this same nerve with the naked eye, or with a weak lens, it presented the white bands as usual.

I at length removed the cellular membrane or sheath of the nerve, without changing its texture, and examined it in this state with the utmost attention, but could only distinguish the winding fibres, as they are described in Fig. 9. However often I repeated this examination, all my researches were useless: I could only find the wavy, winding fibres, when I examined the nerve with strong lens'; and bands or spires, when I observed it with weak ones, or with the naked eye. If my first observations made me believe that a nerve was really composed of large and very white bands, the latter persuaded me that it was formed of parallel winding fibres, so that I could not determine which of these two appearances was the right. It is very true that the first hypothesis seemed to me less likely after the latter observations, because with the strongest lens' I could perceive nothing in the nerve conformable to the first appearances. I could find no remains of a thread which twisted spirally round it, so that it was necessary to examine whether these bands were not a true opticle illusion. In this state of uncertainty,

I saw

I saw no other step to be taken than the continuing to observe this double appearance of bands on one side, and winding threads on the other. I diversified the circumstances of this examination as much as possible, and by acting in this way, if I did not succeed in discovering whence this double appearance of curvilinear bands, and winding threads, arose, every thing however concurred to persuade me, above all when I examined a very small nerve not composed of lesser ones, that the nerves simply contain very fine winding threads, and are wholly composed of them. The microscope would not have been capable of presenting them to the eye in so constant a way, and in so many different circumstances, in which, on observing the nerve with the strongest lens, the winding threads instantly showed themselves, if these threads had not really existed. This successive appearance and disappearance of bands and winding threads, and *vice versa*, persuaded me at length that the appearance of bands, to the eye, either naked or feebly armed, was caused by the winding fibres themselves. By this new hypothesis I was completely enabled to account for the different appearances in the form of the structure of the nerves, and there was no effect which I did not easily explain, nor observation that I did not immediately comprehend. I however sought a more certain proof in a decisive experiment. I wished to take nature in the fact. An obstinate application of several days, which I employed in observations, enable me at length to see clearly, and to discover the whole mystery

tery. Very strong lens' caused the bands to disappear, and very weak ones the winding fibres.

A strong or weak light, directed on the object by the motion of a reflecting mirror, produced sensible changes in the appearances of the nerve ; so that I saw with the same lens, at sometimes the bands alone, and at others the winding fibres alone.

I now employed a lens of a middling strength, and threw a light upon the object in such a way as to distinguish clearly the winding, wavy fibres ; but without seeing the spiral bands. Without touching object or lens, I simply turned the mirror a little, and so directed the light on the object, that at length the spiral bands appeared very distinctly. On scarcely touching the mirror, the bands instantly disappeared, and the winding fibres showed themselves in their stead. I again touched the mirror, when the bands suddenly appeared afresh, and by a motion of the mirror, they once more gave place to the winding fibres. Thus I could at pleasure produce this double appearance, of bands and fibres, by only throwing more or less light on the object.

Figures 9. and 10. represent these successive appearances and disappearances. Fig. 10. shows the nerve stripped of its outward sheath, and viewed with the lens of middling strength. It was so surrounded by light, that the bands or white spots, *c, c, c, c,* and the dark spots, *a, a, a, a,* were distinctly seen. This double class, of dark and white spots, forms the nervous bands.

Scarcely did I touch the mirror, when the bands
suddenly

suddenly disappeared, and I saw in their place the winding fibres of Fig. 9. When I moved the mirror by almost insensible degrees, I observed the bands disappear as leisurely, and the fibres show themselves; or rather I saw the appearance of bands converted into true parallel winding threads: the bands, *c, c, c*, of Fig. 10. became the winding and convex fibres *c, c, c*, of Fig. 9. and the opaque intervals *a, a, a, a*, of Fig. 10. became the concave fibres *a, a, a*, of Fig. 9.

It was no longer possible to doubt the reality of these observations; that is to say, that the bands were not real but apparent; and on the contrary, that the winding fibres were not apparent but real.

All the observations I have made since have confirmed me still more in this, so that I can no longer doubt but that the spiral bands in the nerves are an opticle illusion, and that this illusion is produced by the wavy form of a great number of fibres, or parallel threads, running along the nerve.

Amidst the numerous microscopical observations I have made at different times on animals, or on other small bodies, no one has cost me so much trouble as this, or has been so near to the leading me into an error. I have shown these bands to several persons accustomed to observe the smallest objects, and the structure of the human body; and have met with no one who did not suppose the structure of them in the nerve to be real, and who did not ridicule my assertion, that they could be no other than a simple appearance.

After

After having thus surmounted this first difficulty, and assured myself that a nerve presents to the view a great number of winding fibres, of which it is formed, I proceeded to farther researches. I wished to know what the primitive structure of the nerves is, that is to say, whether it is composed of channels, or of simple threads; whether it merely consists of globules, or contains a non-organick, irregular, spongy matter. This research is as important as difficult, since it tends to nothing less than the fixing, once for all, the ideas of anatomists on the nature of the nerves; that is to say, on the structure of the organ of motion and sensation in animals. They have disputed for more than three thousand years, from Hippocrates down to Albinus, from the Greeks to the moderns, and seem during all this time to have done nothing more than multiply doubts and hypotheses.

Without being very sanguine in my hopes of discovering the first principles of nerves, I have undertaken the investigation with ardour, persuaded that the knowledge I have of their winding fibres, must be extremely useful to me in so difficult a search.

I began my observations on a very small nerve, which I had stripped of the cellular membrane. I observed the winding fibres narrowly with a very strong lens, and determined the size of them. This done, I divided the nerve towards its extremity in a longitudinal direction, by means of a very sharp
needle,

needle, and divided the parts or threads of it, separating one from the other. I immersed the nerve in water, in which the threads floated. After several useless attempts, and several observations either suspicious or inconstant, I at length succeeded in finding many very small cylinders, more or less transparent, seemingly composed of a pellicle, and partly filled with a transparent, gelatinous humour, and with small unequal globules, or bodies. Plate IV. Fig. 3. represents three of these tubes, which I shall call *primitive nervous cylinders*; because these are the parts that constitute the nerve, or its medullary part. Fig. 5. represents another of these cylinders.

To distinguish their structure and shape the better, I examined a great number of these primitive nervous cylinders with a lens that magnified 500 times. Fig. 1. represents one that seemed to have here and there on its outside some fragments of winding threads; and some spheroidal corpuscles in the inner part of the cylinder. Fig. 2. describes another which appeared filled here and there with very small globular corpuscles, immersed in a gelatinous transparent humour. I have seen others which one would have supposed to be filled with a gelatinous substance broken confusedly, and separated into different fragments, so that the gelly of the cylinders may be looked upon as separated, or divided into large, transparent, irregular masses.

However, all the efforts I made to assure myself of the nature and reality of these irregular corpus-

cles belonging to the primitive cylinders, did not enable me to judge precisely of them. They sometimes seemed to me to be spots or irregularities in their external coats, but this I could not determine, and my doubts multiplied in proportion to my observations. I had recourse to a very strong lens, which encreased 700 times the diameter, and after several fruitless attempts, I at length assured myself that the coats of the primitive nervous cylinders were very rugged and full of irregularities. Fig. 4. represents four of these cylinders, *ac, om, rs, ne*, in two of which *ac, rs*, the irregularities are apparent. Being at length assured of this new truth, it remained for me to make myself better acquainted with the true nature of these irregularities, and to know whether they contained globules or corpuscles differently shaped.

To succeed in so difficult a search, I began by separating the primitive cylinders of several nerves, with the point of a needle.

The extremities of the nerves were placed in water, and I ran the point of the needle along them, to break the cylinders, or deprive them in some way of their irregularities: I succeeded at length in meeting with one that had the form described by Fig. 6. About half of this cylinder *ac*, was formed of a transparent and uniform thread, and the other half *ma*, was almost twice as thick, less transparent, irregular, and rugged. I then suspected that the primitive nervous cylinder was formed of a transparent cylinder, smaller, and more uniform,

uniform, and covered with another substance, the nature of which was perhaps cellular.

The observations I made afterwards confirmed me invariably in this hypothesis, which at length became an established fact. I have very often seen these two parts, that compose the primitive nervous cylinder. The exterior one is unequal and rugged; the other a cylinder which seems formed of a particular, transparent, and homogeneous membrane, that appears to be filled with a gelatinous consistent humour.

Fig. 4. represents, as has been seen, a group of these primitive nervous cylinders, in the way I have observed them on examining the nerve of a rabbit. One of these cylinders, *o, m*, was entirely stripped of its external and rugged membrane, and had an uniform transparent appearance. Another was in a like way stripped, except at one extremity, *n, e*, which seemed covered and enclosed by an external rugged membrane. A third, *ac*, was almost altogether covered with this rugged membrane; the fourth, *r, s*, entirely so.

Fig. 7. represents a primitive nervous cylinder in which, *o, r*, the thick part, is covered with a cellular membrane composed of fine threads. The part, *r, s*, is stripped of this cellular membrane.

On examining this external covering of the primitive nervous cylinders with attention, it seemed to be composed of winding threads, running
along

along the nerve, and so forming a cover to the interior cylinders. I was well assured of this soon after, by employing a lens that magnified 800 times. Fig. 8, represents a primitive nervous cylinder, covered with its exterior sheath, which is distinctly composed of very small winding threads; running along the cylinder.

The progression of these threads may in some measure be compared to the canal formed by the epididymis, which produces incessant folds. The threads are very small, not appearing to be more than $\frac{1}{13000}$ part of an inch in thickness; and although they are thus fine, they form so thick a coat round the primitive nervous cylinder, that they almost triple its diameter; this is caused by their winding and heaping upon each other exceedingly.

These winding threads, which cover the primitive nervous cylinders, I shall call the *winding cylinders* of the nerves; and considering them collectively, as a wrapper to the above primitive nervous cylinders, I shall call them the *external sheath* of these cylinders.

Fig. 9, represents a primitive nervous cylinder, covered with its external sheath. The sheath is woven with winding threads, some of which are a little separated from others by the point of a needle. These winding threads have a perceptible thickness when viewed with very strong lens, although much finer than the primitive nervous cylinders.

The primitive construction of the nerves is as follows: a nerve is formed by a great number of transparent,

parent, homogeneous, uniform, very simple cylinders. These cylinders seem composed of a very fine uniform tunick, filled, as far as the eye can judge, with a transparent, gelatinous humour, not soluble in water. Each of these cylinders receives a cover in form of an external sheath, which is composed of an immense number of winding threads. A very great number of transparent cylinders form together an almost invisible nerve, presenting the exterior appearance of white bands ; and several of these nerves united, form the larger nerves seen in animals.

I am fully convinced by my own observations, repeated a great number of times with the same success, that the cylinders I have described, are the simple and first organical elements of nerves, for I have never been able to divide them farther, whatever trials I made with the sharpest-pointed needles. I could easily tear and rend them here and there, but they always remained simple as before. I could strip them of their sheaths, and separate the winding cylinders of which these were formed, although they were very small. The primitive nervous cylinder then appeared transparent, homogeneous, and every where of equal diameter. We see by this how much even the best anatomists were in general mistaken, when they maintained that the nerves were divided and subdivided without end, without there being any hope of ever seeing, or coming at a knowledge of, their first threads, or first organical elements.

This

This seems to me to be a great stride towards the understanding an organ so essential to life, and which till now has been hid to the eyes of the greatest observers.

I reserve the examining, till I am more at leisure, the matter of which the nerves are composed, or filled. This subject when well known, may throw a very great light on the knowledge of the animal economy, and perhaps likewise, on that of a great number of diseases.

ON THE STRUCTURE OF THE BRAIN.

AFTER having examined the structure of the nerves, and their first organical elements, order requires me to turn my attention to the brain, whence they draw their principle origin. We know that the brain is composed of two substances, named cortical and medullary, which are distinguished by their respective colour.

I have already related the various opinions of authors as to the structure of the brain. Some believe its substance to be altogether vascular, and others will not have it to be so. There are some who suppose it simply composed of blood vessels; others on the contrary believe it formed of vessels much smaller again than these.

Malpighi believed the brain, as well as all the other viscera in the body, destined for particular secretions, to be glandular. Of the more modern ob-

R.

servers,

servers, some believe it to be formed of simple globules, others of a non-organical and spongy pulp.

The observations I had made on the medullary substance of the nerves, were a great help to me in examining the brain, although in the cortical substance I encountered very great difficulties, which I should never, perhaps, have been able to surmount, if I had not been previously acquainted with the medullary substance of this organ, in which every thing is seen much better, and in a clearer way. 'Tis for this reason that I shall begin my observation by examining the medullary substance of the brain.

I shall not speak of the blood vessels that are seen in the medullary substance of this viscus, and which are well known to anatomists, particularly since the use of injections.

I shall only treat of that part of the medullary substance which is quite white, and which is certainly not formed of blood vessels. I cut a small and very thin lamina of this medullary substance, and extended it on a glass wet with water. I examined it with the greatest attention, purposely varying the light, and it seemed to me to be formed of a transparent cellular substance, covered here and there with very small, rounded, winding cylinders. Plate V. Fig. 8, *rr.* represents this substance, which, when well observed, seemed composed, as it were, of a heap of intestines; but all was obscure, and uncertain. At its side, at *a a*, there were several corpuscles, detached from the cellular substance, swimming in the water. Some
ones

ones were larger than the others, they were irregular, and more or less oviform. This intestinal form of the medullary substance of the brain, made me suspect that there might be canals or vessels, and that it might be wholly formed of them. And in reality, having observed this medullary substance afresh, as in Fig. 8, with a much stronger lens, it appeared absolutely formed of an heap of small, irregular, winding, transparent intestines, filled with a gelatinous humour. It was not possible for me to discover any more ; it only appeared that these small intestines were very short, and that some of them terminated in small globules, or spheroidal bodies.

However certain this observation appeared to me, it still left me a great deal to desire. I wished to see things clearer, and to assure myself that these were canals, which ramified. I employed a lens that magnified in diameter upwards of 700 times, and after several fruitless attempts, I at length succeeded to observe the part I had before touched with the point of a needle, and which I had wetted afresh with water. I then found to a certainty, that it was really formed of a winding and vascular substance, which folded as the intestines do, making several turns and circumvolutions. Fig. 9, exactly represents its manner of appearing to the eye, armed with a microscope. Globules, which seemed surrounded by something, were spread about it ; certain round, or blunt bodies, appeared in the intestinal substance itself ; and some of these intestines

seemed to terminate in these bodies. It is however certain, that round corpuscles are tenaciously fixed to this substance, and that they are detached with difficulty by water.

The point of the needle, however, had detached several bodies from this substance, which are represented by Fig. 16. Some of them, particularly the largest, appear to be branched ; others seem to end in the corpuscles I have spoken of. I can nevertheless form no conjecture on them, and only represent the object as I have seen it. I have repeated this observation a great many times, but have not yet succeeded in seeing any thing more. 'Tis true, I have assured myself to a greater certainty, that the medullary substance of the brain, is not a simple collection of venous and artereal vessels ; that it is not simply formed of spheroidal globules or corpuscles ; but that it is an organised, particular substance, composed of irregular cylinders, or transparent canals, which fold as the intestines do, and which I shall call the *intestinal substance*, on account of the shape in which it is seen.

This particular, intestinal, substance, of which the marrow of the brain is formed, is not soluble in water, any more than is the transparent matter with which these intestines seem to be filled. The round corpuscles likewise, that have been described, are not capable of being dissolved in water.

Such is the structure of the medullary substance of the brain, and in this way I have found it in all the animals I have examined. My observations on

this subject conclude here, and I can venture to say, that I flatter myself with not having been deceived. All that I could further advance on this occasion, would be nothing more than conjectural hypotheses.

Cortical Substance.

The intestinal structure I had discovered in the medullary substance of the brain, assisted me in discovering something like it in the cortical substance of this viscus. I say something like it, because I could not after all trace the progress and intestinal structure of this substance in so clear a way, as I had done that of the medullary one.

I began to examine the cortical substance, in the way in which I had examined the medullary one, viewing a thin lamina of it moistened with water. Fig. 6, *rr*. represents this lamina observed with a very strong lens. It seemed to be formed of an irregular web, granated here and there, which I should have supposed a cellular membrane, if I had trusted to a simple inspection. Beside this substance, were very small, irregular, transparent, spheroidal corpuscles, which seemed filled with a gelatinous humour, and which were not soluble in water; they were smaller than those I had observed in the medullary substance, but in every other respect like them. I now had recourse to a still stronger lens, with which I at length succeeded in observing a structure altogether like that I had observed in the

medullary substance; that is to say, composed of a transparent, organical, vascular substance, formed like the intestines. On observing Fig. 7. its revolutions and windings in this substance, like those in the medullary, will be seen at *m, a*; it only seems to differ from the latter in the fineness of its vessels, which it is very difficult to see distinctly. About it were the corpuscles, *r, r*, which likewise seemed to be surrounded by something. The observations I have since made, have more and more convinced me, that the structure of the cortical substance of the brain, is as I have described it, and that it does not differ sensibly from the medullary substance, although their colour makes them appear so distinct. I do not pretend to deny, but that the uses of these two organized substances may be a little different; and the different dimensions of their respective intestinal substances, give great reason to suspect it.

I wished to see the union of these two substances, and to observe the end of one, and beginning of the other; but could not succeed. It however appeared to me, that the fluid with which the intestines of the cortical substance seem to be filled, agrees in its nature with that of the medullary substance.

Retina.

After having examined the primitive structure of the nerves, and those of the cortical and medullary

dullary substances of the brain, I thought it proper to bestow a particular attention on the parts in which the nerves terminate, or where they become the organs of a particular sense in an animal. For this purpose I made choice of the retina, which appeared to me the most proper organ for my purpose, and the use of which is the most noble. I reserved to another occasion, the examining the nervous expansions directed to the other senses.

Authours are divided amongst themselves as to the primitive structure of the retina, although they agree in general in other respects; as to the nature of this organ, which they believe to be formed of the medullary part of the optick nerve. Several have believed it to be a pure web of nervous fibres, the primitive size of which; as may be seen in the works of Portenfield and Gesner; they have even gone so far as to determine; but the existence of these fibres, and their size, in these authours, are rather founded on theory, than on immediate observation; so that a nice observer, who does not content himself with hypotheses, can have but little dependance on their calculations.

There are others indeed who have struck into the path of observation, but their researches have been very superficial, since they have contented themselves with simple ocular inspection, or with very common lens'. It hence has arisen, that they have given us nothing more than vague and general opinions, and have considered the retina as

no other than an expansion of the medullary part of the optick nerve.

But others again, more venturous, and accustomed to observe very minute bodies with very strong lens', have carried their researches still farther, and have assured us that the retina is not formed of distinct nervous fibres, but rather of the mucous substance of the brain, which several of these observers imagine they have discovered, and which, as we have said above, is nothing more than a mass of very small simple spheroidal corpuscles, or as others have thought, of a confused non-organick pulp, or of simple threads, and cellular lamina.

Some observations I had previously made on the retina of rabbits, were very useful to me in the examination I afterwards entered upon, of the retina of other animals. Had I not known the qualities of the former, I should perhaps like others have been deceived, and should have denied a structure to this organ, which is absolutely proper to it.

The retina of rabbits observed with the naked eye, appears very different from that of other animals viewed in the same way. A particular structure in the former instantly fixes the attention of those who observe it. In the inner part of the retina, (see Fig. 12.) and opposite the lower entry of the optick nerve, a pretty deep hollow, the edges of which are well raised, forms itself; it is wider towards the head and upper jaw, than towards the two corners of the eye. From the bottom of

t h is

this hollow arises a very great number of exceedingly small and very white nervous threads, which spread all around it, as rays that diffuse themselves from a common centre, and form the retina.

If the optick nerve be divided lengthways with a scalpel, into two equal parts, the threads of it, which are no other than a continuation of the above mentioned filaments, correspond perfectly with them. These nervous filaments on their leaving the hollow, continually diminish in size, and ramify; in proportion as they are more distant from their origin, they become more numerous, till at length, being so very fine as to be scarcely visible, they terminate in very subtle threads at the beginning of that part of the retina which I shall call *muscos*, to distinguish it from the other, which I shall call radiated, or *filamentous*. The nervous rays diffuse themselves in every direction round the hollow, but are very long in two parts, *r, r*, diametrically opposite to each other, and occupy the greater part of the internal portion of the eye, so that the unradiated part of the retina is very small in this place, in comparison with the other two parts which correspond with *m, m*.

These small nervous filaments, when very nicely examined, are of a crooked structure; and have the usual appearance of spires or bands, above all at the other parts where they are least divided; but this is very difficult to be distinguished clearly.

These

These small nerves, or radiated fibres, which are so easily distinguished in the eyes of rabbits, even without the help of glasses; are observed with great difficulty in the eyes of other animals. I candidly acknowledge, that if I had not first seen them in rabbits, I probably should not have found them in other animals, since not having any suspicion of them, I should not have examined the retina with all the attention that is necessary to distinguish them. I must likewise observe, that it is very difficult to view these radiated fibres distinctly, and to verify their existence; to see them with facility, very strong lens' must not be employed. The best are those that scarcely magnify six or eight times. The retina must likewise be examined at the entry of the optick nerve, where the medullary part of the nerve is the largest. It is only distinguished well and to a certainty, in some lights. I have succeeded in distinguishing it still better when I have thrown the marine or some other acid, well diluted with water, upon the retina, which then becomes white and opaque, and the nervous fibres are more clearly seen in it. Sometimes I have viewed them very well, after throwing some drops of water on the retina at the entry of the optick nerve. Although I am very certain of the existence of these small radiated nerves in the eyes of all the animals, such as oxen, lambs, and kids, that I have hitherto examined, I shall however be not at all astonished, should they be hidden to the eyes of many observers, even to
some

some who best understand the use of the microscope, and view the minutest objects in the best manner. These small nerves do not escape the eye on account of their extreme fineness, but rather on account of the great conformity they have to the pulp of the retina itself, which covers and conceals them, if I may be allowed the expression, from the observer. They appear to me to be of the same length, in the eyes of all animals, except, as has been seen before, in those of rabbits, and they disappear when arrived at two thirds the space that is found betwixt the entry of the optick nerve, and the *plexus ciliaris*, so that the unradiated retina in these animals only occupies one third, or thereabout, of the orbit of the eye.

These radiated nervous fibres are very numerous, and seem to be formed of, or covered with, a medullary, dark, slightly transparent, pulp, which when examined with the strongest lens', seems to be composed of very small transparent spheroidal bodies, well united together, and tied as it were by very fine transparent membranes or filaments.

That part of the retina in which the small nerves are seen in a radiated form, ought then to be considered as composed of two parts, one radiated, the other pulpous, or simply medullary. The radiated part of the retina, if the eye be examined at the pupil, is covered with a particular substance, like a non-organick mucus, and the part beneath this mucus is formed of small, decreasing, longitudinal nerves, that is to say, of nervous rays.

I found

I found the unradiated nervous part of the retina, to be likewise composed of very small nervous globules, supported by a very fine transparent cellular web, in which it seemed in some way to enclose itself. These globules are smaller than those of the blood. I found them in rabbits to be about $\frac{1}{3500}$ part of an inch in diameter, whilst those of the blood were the $\frac{1}{2500}$ part. The globules, which appear to be formed of a transparent gelly, in the retina of rabbits, do not dissolve in water like the red globules of blood, and are strongly attached to a cellular substance, that seems to support them. Fig. 11. represents a small portion of the retina, with the globules and cellular web.

The sizes and respective forms of the globules of the nervous part of the retina are represented by Figures 10. and 11. The small bodies in Fig. 10. represent the globules of the retina, and those of Fig. 13. represent the globules of the blood.

The globules of the retina have a great resemblance to those that are found in the brain. Water and acids produce the same changes in them; they are equally transparent; and I have only remarked, that the globules in the retina are more regular and uniform than the others.

When the retina is kept a long time in water, and wiped a little, shreds of it are often found more or less deprived of those globules, and it appears in these parts like an unequal, rugged, cellular web, formed with small hollows capable of receiving

receiving these globules. Fig. 15. exactly represents the retina in this state.

Thus appears the retina when nicely observed, and we are scarcely permitted to penetrate farther into its nature. The part which corresponds to the entry of the optick nerve, and which extends a great way, is composed of very fine threads of nerves, and of a nervous pulp formed of very small transparent globules, attached to a very loose, transparent, and rugged web.

The other part of the retina is simply formed of the usual globules, and cellular web; and as far as I could observe, seems to have no nervous threads in its composition.

Wishing to be better acquainted with the nature of these cellular webs of the retina, and how the globules of the medullary part are attached to them, I began afresh to examine the retina with very strong lens', and have at length, after many attempts, been able to distinguish more precisely. I think I may venture to advance that these cellular webs are no other than a net-work of very small transparent crooked vessels, to which, as is seen in Fig. 14. these globules attach themselves. These crooked vessels resemble very much, in transparency, figure, and progress, those of the medullary substance of the brain, and they only seem to differ in their size, which is somewhat smaller, so that there seems to be a particular intestinal substance, which is found in all these parts.

Since

Since the experiments of Mariotte, and the calculations of Daniel Bernouilli, we know that the part of the retina which corresponds to the entry of the optick nerve, is blind, that is to say, that the images of objects expressed there, are in no way perceptible to us; and this nervous part in man is nothing less than a small disk of the diameter of a paris line. The nervous fibres in this place, are larger and more heaped together than in any other parts; the pulp is likewise larger and more heaped. However incredible it may appear, it is certain that the small nerves and pulp of the retina I have just mentioned, are not organs of sight, and that they only become so in the part where all is finer, more rare and more open. Is this insensibility of the retina to the light, caused by the nerves being as yet too large and not well freed from the cellular membranes? Or is it caused by the pulp of the retina being too much heaped, preventing in this way the rays of light from reaching these nerves?

But it now remains to make another very important search, by examining whether the Retina is in all its parts sensible to external objects.

At a small distance from the entry of the optick nerve it is as has been described certainly so, and it clearly continues to be so at a great distance from the same nerve; the rays of light likewise extend themselves to a great distance, when we regard substances in a natural way; but let me enquire whether this vision is produced at the spot where the rays end,
and

and the mucous part of the retina begins. Do external bodies there cause a sensation? In a word, how far does the organ of vision extend? The solution of these problems, however difficult they may be, is not impossible. It depends on an exact knowledge of the parts of the eye, and a few experiments purposely made; I however have not time to busy myself in this search. It is generally known that in proportion as images are expressed at a greater distance from the optick nerve, the objects are seen more confusedly; so that there is in all appearance, a spot or limit, at some distance from the optick nerve, where vision is the most distinct, without our being yet able to assure ourselves whether it be at the precise place where the nervous part of the retina begins.

The blood vessels of the part, and principally the venous ones, are generally covered with the nervous fibres of the retina, and with its mucous substance. They are at least very often observed to be so in the eyes of oxen; but these vessels are in many places totally deprived of every nervous substance, and in these parts frequently cross the retina, and its adjacencies, breaking its order and texture, and so rendering it in many places insensible to external objects, although it does not appear so to us, on account of the great mobility of the eye.

Fig. 10. and 11. of Plate IV. represent two singular canals, with intermissions here and there. It may be suspected that they are lymphatick vessels; above all that of Fig. 11. I cannot decide as to their

their nature, having so seldom met with them. I found these two vessels, in examining the substance of the brain.

It remains for me to make an observation or two, on certain figures of Plate V. which have been improperly introduced, by an error of the engraver, into that plate.

Fig. 1. represents several oviform bodies of different sizes, which are found in the external cellular coat of the nerves.

Fig. 2. represents very small corpuscles which I have observed in examining the medullary substance of the nerves. In Fig. 3. are other oviform bodies, which are globules of the blood of a rabbit, and lead to a comparative judgment of the respective sizes of one and the other.

Fig. 4. represents several winding cylinders of the adipose membrane.

Fig. 5. represents two threads, *m*, *a*, one placed at the side of the other, to show their respective sizes, *m* belongs to the adipose membrane, and *a* to the external cellular membrane of a nerve. They are of an equal size.

ON THE STRUCTURE OF THE TENDONS.

THE observations I made on the structure of the nerves, and of the undulating progress of their primitive cylinders which causes the uncommon appearance of bands I have before described, stimulated me to examine with all possible attention the structure of the tendons. I did not find any difficulty in observing a certain spiral form in them, although it did indeed appear less regular to me than in the nerves. This apparent spiral form is observed, not only in regarding externally the larger tendons, but even the very smallest of them. These bands however, when better examined, have rather the appearance of longer or shorter winding spots, which a nice observer will easily distinguish from the bands that are seen in the nerves, and which we have described. When a tendon is ex-

mined with a lens that magnifies but a few times, white spots are perceived through the cellular membrane that covers it, as they are represented by Fig. 1. Plate VI. in which the tendon is magnified six times. Fig. 2. represents another tendon likewise observed with a very weak lens, and in which the spires, or small curvilinear spots, were more regular, and very much resembled those observed in the nerves. This spiral structure of the tendons is likewise observed with the naked eye, though not so distinctly as with a microscope.

My principal attention was to examine nicely the elementary threads of the tendons, their size and their progress. I at first suspected that their progress was analogous to that of the primitive nervous cylinders, and that the small white curvilinear spots owed their origin, or their apparent existence, to it.

This last research seemed to me the more important, since it tended to decide whether, besides the whole nervous system, other organical parts were to be found in an animal, of an undulated and winding texture, like that of the elementary parts of the nerves.

All the tendinous substance in general, or rather all the tendons, when examined with a microscope, seemed to be formed of a vast many very small, simple, longitudinal *fasciæ*, separated one from the other by the cellular membrane. Each of these *fasciæ*, which I shall call *primitive fasciæ*, because they are not composed of smaller ones, is formed of

an infinite number of extremely fine threads, which I shall call *primitive tendinous cylinders*, because they are not subdivided into smaller ones, in whatever way they are prepared or examined. These primitive cylinders run along the tendon for its whole length, and are solid throughout; that is to say, neither vascular nor hollow. They are much smaller than the primitive nervous cylinders, and are tied together in the primitive tendinous *fascia*, by an almost imperceptible, subtle, and clastick cellular membrane. These primitive cylinders appeared to me of the same size throughout the whole of the tendon in an animal, and likewise in all the tendons. They are homogeneous cylinders, every where uniform, neither hollow, nor formed of small vesicles or globules; in a word, they are canals.

All the researches I made to discover whether these cylinders were composed of other smaller parts, were totally fruitless, so that I am obliged to consider them as non-organick, primitive solid threads. These primitive cylinders then, which compose the tendinous substance in its last decomposition, being many of them united together, form the primitive tendinous *fasciæ*; and it is from several of these last that the tendon is at length composed. As the cellular membrane which binds together the primitive tendinous cylinders yields easily, and as at the same time that of the primitive *fascia* itself is transparent, it is easy to distinguish the progress of the primitive tendinous threads, and this progress

is so very like that of the primitive nervous cylinders, that it would be difficult to distinguish them. The tendinous threads elongate, and form undulations in the whole substance of the tendon, and the appearance of a spiral structure and of bands in the tendons, as well as in the nerves, is caused by these undulations.

Fig. 3. represents a primitive tendinous *fascia*, which appears formed of a vast number of primitive tendinous threads. These threads, parallel to each other, run along the tendon, forming regular undulations, from whence, as has been said, arises the appearance of bands or spires. At *r, r*, two of these threads are seen, purposely separated from each other with the point of a needle.

Fig. 4. represents another tendinous *fascia*, composed of primitive threads, *r, r, r*, observed in water, and stripped of their cellular membranes. The cylinders here are neither undulated nor winding, because they have been pulled out, and deranged from their natural situation by the needle with which I separated them.

My researches into the structure of the cellular membrane of the nerves, which, as we have seen, is no other than a web of very small, winding, transparent cylinders, led me to think that the cellular membrane of the tendons should be of the same nature, that is to say, quite filled and woven with the same cylinders. I have in reality observed them with very little difficulty, and found them of the

the same size and form as in the nervous membrane, and their progress to be alike in both.

Fig. 5. represents a small portion of the membrane, or cellular web, of a primitive tendinous *fascia*. It was formed of a great number of winding cylinders, of which a few only are expressed in the engraving, that they may be the better distinguished, and are indicated by the letters *r, r, r, r, r, r, r*. I observed them with the same lens with which I observed those of the cellular membrane of the nerves, and the primitive tendinous threads of Fig. 4. and 5. explained above. The sizes of these threads of the tendon, and of the cylinders of its coat, are equal to those of the winding cylinders of the nerves, and almost equal to those of the primitive tendinous threads themselves, so that they may be all regarded as pretty alike in size, without falling into any sensible error.

On the Tendinous part of the Diaphragm.

Fig. 1. of Plate VII. represents a portion of the diaphragm of a rabbit. *a, p, q, r*, is the fleshy part; *a, m, c, r*, the tendinous part; *n*, is the trunk of a nerve that enters the diaphragm, and *a, r*, is a vein. What deserves attention is, that the nerve *n*, has all its ramifications towards the fleshy part of the diaphragm, and none towards the tendinous part. I have however found nerves in other animals,

mals, which led towards the tendinous part; but they had no further ramifications in their progress towards it; and I have not till now observed in any case, any nervous branch which ended in the tendinous part, as happens in the fleshy one, where they decrease rapidly and disappear. *f, f, f,* are the branches of the nerve *n*. *o, o, o, o,* are the branches of the vein *a, r,* towards the fleshy part. *y, y,* are very small branches of the same vein, running in almost a straight line along the tendinous part of the diaphragm, and forming, as is seen, a very few smaller branches. *u, u, u, u, u, u, u,* are very small longitudinal vessels without ramifications, which, rising from the vein *a, r,* run along the tendinous part.

The tendinous substance is bright like silver, and transparent; and its branches, even the smallest of them, are opaque. If the smallest thread of a nerve were received in it, it would be easily seen; and this very different mode of ramification which the nerve assumes in the two different parts of the diaphragm, is as complete a proof that the tendons do not receive nerves, as it is certain that the muscles do receive them; it is a complete proof that these two animal substances are perfectly different from each other; and it proves the falsity of one ever having been, or having ever degenerated into, the other, as many anatomists have believed.

The physical reason why the nerves do not ramify towards the tendinous parts of the diaphragm, and why the vessels send very few ramifications,
and

and those scarcely perceptible, to these parts, may, I think, be principally ascribed to the substance of the tendons itself, which presenting a greater obstacle than the fleshy part, does not allow a great and free vegetation, either to the nerves or vessels.

When the tendinous part of the diaphragm is nicely examined, as well with the naked eye as with glasses, the usual small spots and bands are observed in it, formed in the accustomed way, of undulating threads. Fig. 2, represents a very small portion of this part of the diaphragm, in which, observed with a very strong lens, is seen the undulated progress of the primitive tendinous threads.

The ramifications of the blood vessels likewise deserve some consideration. The vein throws out all its principal branches towards the fleshy part of the diaphragm, or rather, its principal ramifications; and these are very small. A vast number however of very fine vessels, without the usual ramifications, and almost parallel, run in a right line by the tendinous part, and pass into the opposite fleshy part, where they ramify, and at length lose themselves.

On the Structure of the Muscles.

My observations on the tendons led me to examine the muscles, or rather their elementary fibres. The structure of the muscles, when observed, is more regular and less uncertain than that of the tendons, although observers do not agree as to all

the particulars. The learned Prochaska deserves here again a particular homage, for having supplied us with a small work, entitled, *de Carne Musculari*: Vindob. 1778, in which he leaves us very little to desire on this subject.

This skilful professor finds an appearance of whitish wrinkles, on examining the muscular fibres with a very strong lens, and is persuaded that they are no other than superficial impressions made by the vessels, cellular cylinders, and perhaps likewise the nerves, which surround the sheath of the muscular fibres themselves. He is of opinion, that when a muscle is boiled in water, these vessels and very delicate threads shorten, embrace the fibre here and there, and imprint on it the whitish wrinkles or crevices.

Prochaska represents the appearance of these wrinkles by Fig. 12, of Plate IV. in his work, and this Figure is perfectly similar to Figures 1 and 2 of Plate VI. of mine.

I observed in breaking the muscle by degrees, with very sharp-pointed needles, that it is at length brought into very fine threads, which no pains can divide again into lesser ones. I shall call these filaments *primitive fleshy threads*,

Some hundreds of these threads united together, form a simple fascia, which I shall call *primitive fleshy fascia*. The muscle is at length formed of a great number of these fasciæ.

I examined these fleshy fasciæ with all possible attention, and with lens' of $\frac{1}{40}$ part of an inch in the
focus,

focus, but have never succeeded in finding a structure altogether similar, either to that of the tendons, or that of the nerves. The greater part of the small white spots that ran transversely across the fascia, were curvilinear, semicircular, uniform, and uninterrupted.

Fig. 6, Plate VI. represents four primitive fleshy fasciæ, in contact with each other, and covered with their cellular membrane. Two *m, m, s, s*, are, as I have said, spotted circularly, and in the two others, *r, r, a, a*, the spots seem in some places to form a certain number of small angles, such as are described in the Figure. This is all I could observe to a certainty.

Fig. 7 represents a primitive fleshy fascia, covered like the four others, with cellular membrane, but only in part. I succeeded in raising this cellular membrane, as it is described in the plate, at one of its extremities, and in observing the primitive fleshy threads, and at the same time, the small circular spots. The primitive fleshy threads are solid cylinders, equal to each other, and very perceptibly marked at equal distances, with resemblances to small crevices or wrinkles. I could not perceive a true undulated progress in these threads, and the small curvilinear spots of the primitive fascia, appeared to me to be occasioned by the small indented marks of the primitive fleshy threads. *m, o, r, c*, is the part still covered with the cellular membrane; *a, e*, are primitive fleshy threads, separated from each other.

Fig.

Fig. 8, represents a primitive fleshy fascia, covered with its sheath. After several attempts, I succeeded in stripping it entirely, as it is drawn in Fig. 9. It was composed of a very great number of solid homogeneous cylinders, which were interrupted at equal distances, by very small marks or lines, which, observed in different positions, might have passed for small globules. I cannot decide as to their true nature, observation not carrying me further. Sometimes one would suppose these apparent globules to be so many wrinkles, formed by the contraction of the threads themselves.

I observed them, both immediately after the animal's death, and when they were on the point of becoming putrid. Fig. 9, does indeed represent these cylinders a little undulated, and so they appear to the naked eye; but they could not be in their real state, after the preparation they underwent. The letters, *r, r, r, r*, express primitive fleshy threads, a little separated from each other, and forming a reunion at *a*.

These successive observations forced me to allow some difference betwixt the progress of the tendinous cylinders, and that of the fleshy cylinders; the appearance of spots in the primitive fleshy fasciæ likewise seems a little different from that of the spots of the tendons.

I also paid a particular attention to the coats and cellular membrane of the muscles, and found them to be formed, as in the tendons, of the usual transparent winding cylinders. Fig. 10, represents a
small

small portion of the cellular membrane of the muscles, which is seen to be no other than a web of very small cylinders. *m, m, r, r*, shows their progress and size, which are exactly the same as in the tendons and nerves,

Difference betwixt Nervous, Tendinous, and Muscular Substances.

After all that has been said on the structure of the nerves, muscles, and tendons, these three substances ought readily to be distinguished one from the other. It has been seen that the nerves are composed of transparent primitive cylinders, which appear to be filled with a mucous substance. These cylinders are much larger than the primitive fleshy cylinders, and primitive tendinous cylinders, so that it is absolutely impossible to confound them together. The primitive nervous fibres have another characteristick in their progress; they run serpentine and undulating, whilst the fleshy fibres tend very much to a right line.

The primitive fleshy threads of the primitive nervous cylinders are instantly distinguished, not only by their being so very small, and by their progress, but likewise by their solidity. They are in no way either vessels or canals, but solid cylinders, homogeneous throughout. The apparent structure, not only of the simple primitive fleshy thread, but
likewise

likewise of the fleshy fascia itself, is besides altogether different; and when we are a little accustomed to examine them, it seems no longer possible to be mistaken, and to confound one with the other. It is true that the primitive tendinous threads have an undulated and winding progress, like the primitive nervous canals, but they are much smaller, and like the fleshy threads, are quite solid, so that it is impossible to mistake them for the primitive nervous cylinders, which seem to be filled with a distinct substance. Neither can the fleshy threads be easily mistaken for the tendinous threads, although both of them are solid and of equal bulk, because the latter are evidently distinguished by their winding progress, which is not seen in the fleshy threads; and because they do not alter in size or shape on their progress, in opposition to the fleshy threads which are interrupted continually by small crispations and nodosities.

These characteristicks being once well established, I repeat that it is no longer possible to confound these three animal substances, the nervous, the fleshy, and the tendinous, together. I have many times made the proof of this, without ever having been deceived. I employed a person to put the smallest imaginable particle of a nerve, a muscle, or a tendon, at his choice, under my microscope; these particles were detached with the point of a very fine needle, from the above substances swimming in water; a moment's examination enabled me to
know

know them to a certainty, and to distinguish to which of the three substances they belonged.

It would be superfluous to dwell on the importance that the distinctive characteristicks I have fixed on the primitive structure of the nerves, muscles, and tendons, may be of to anatomy, and to animal physicks. It is still doubted whether many of the parts endued with motion, have, or have not, muscles. No one can be ignorant of the dispute on the fleshy fibres of the uterus, particularly on the existence of the *musculus orticularis* of Ruisch. When we see an Albinus pass this muscle in silence, when speaking of the uterus; a Haller who cannot find it; and Ruisch himself who appears to disavow it in his old age, we remain in suspense on viewing the excellent researches a great English anatomist has made into this muscle (a).

All the difficulty lies in knowing whether what is by some called a muscular substance in the uterus, is really so. The existence of a substance in this viscus is incontestible, and this substance by some is said to be fleshy, whilst others openly deny it that quality. A very small particle examined by a good observer in a microscope will decide the question. The characteristicks of the fleshy fibre are too clear to be confounded with other animal substances. The nature of it may then be determined in a few instants, and the question, which to the great scandal of anatomy has existed for half a century, may be decided.

(a) Hunter *de Utero Gravidæ*.

The

The same may be said of the other parts of the animal machine, and a like trial may be made, when there is a doubt whether any particular part does, or does not, receive tendinous fibres. The characteristics of these last are no more equivocal than the fleshy fibres, and a microscope may determine to a certainty whether such parts are tendinous or not.

In a word, I believe the well establishing the characteristics of the three substances, the nervous, fleshy, and tendinous, to be highly advantageous; and if I had found myself in favourable situations, I should already have made applications which would have been useful to the knowledge of the human body, and should probably have dissipated many doubts, and terminated many questions, as to the structure of many of its parts. Wherever, for example, there are tendinous fibres, it would not be difficult to explore them, and to ascribe to every part the texture it really has.

I was desirous in the mean time of examining, according to the rules I have established above, what the structure of the small blood-vessels is; and in spite of my attention and patience in the course of my observations, I have not been able till now to see any thing that might lead me to suspect, that there were either nerves or muscles in the textures of their coats. I do not pretend however to assert any thing decisive on the subject, and even wish other observers to busy themselves upon it, and to see whether or no I have been mistaken. But they will in the mean time allow me not to admit of those
theo-

theories that have a nervous or muscular structure, of which they suppose the blood vessels to be formed, for their basis, and which they do not see.

Many things have been written, and most of them uncertain, on the ganglions. It is generally believed that they serve to collect the medullary part of the nerves, and to furnish an origin, as if each of them were a small brain, to new threads of nerves. By entering upon a short investigation of the ganglions, a good observer may easily know the structure of these organs, and form a truer conception of their uses, which seem to be very important to the animal economy.

Were I in circumstances more favourable to this species of observations, I should not fail to examine all these parts, and still many others, which I am obliged, for the present however, to leave to the industry of other observers. I shall only say two words on tendons. Anatomists dispute whether they be or not, an elongation of fleshy substance, that is to say, of the same nature as muscular fibres. I can take upon me to say that I have never seen a primitive fleshy thread, nor a primitive fleshy *fascia*, become tendinous, however I have multiplied my observations, particularly on the tendinous and muscular parts of the diaphragm, in small animals. I have seen primitive fleshy *fasciæ* terminate as a fleshy substance, and have seen primitive tendinous *fasciæ* introduce themselves betwixt the fleshy *fasciæ*, but it was not to unite their substances

substances with that of the others. In a word, one *fascia* does not begin where the other ends, but they introduce themselves into each other, like the cogs of two wheels that fasten and rise one within the other; the tendinous threads particularly, penetrate a great way amongst the muscular ones.

*On the primitive winding Cylinders of the Animal Body,
or on the Cellular Membrane.*

The primitive winding cylinders I have discovered in the cellular membrane of the nerves, tendons, and muscles, are of all the parts or organs that I know of, in the animal body, the smallest. They are, as has been seen, much smaller than the finest blood vessels, that do not allow but one globule of blood to pass at a time. All the attempts I have made to reduce them into smaller cylinders, have been ineffectual, and although they are observed with glasses of the greatest strength, they appear very simple, and not surrounded by other smaller vessels.

The philosopher who is not fond of hypotheses, who admits no other structures or parts in the animal body as certain, than those which observation has discovered in it, will find no difficulty in considering these winding cylinders, as primitive simple principles, not composed of other lesser ones. This is a *datum* of which observation demonstrates

demonstrates the reality, and from which we must set out, to reason fundamentally on the functions of the organical parts of a living body.

One general purpose of these winding cylinders may be, if they are really vessels; to nourish the parts in which they are found, or which are surrounded by them. According to this hypothesis, they may perhaps serve for the nutrition of the primitive cylinders, as well nervous, as tendinous and fleshy. But there is another purpose, still more noble, and perhaps equally important, to be ascribed to them; the principal functions of life may even depend on them: the smallest changes made in these organs, may cause the utmost disorder in the animal economy.

My experiments on poisons have shown that they bring about the death of an animal in an unknown way, and we seem to need the discovery of a principle, an organ in short; on which these poisons act. Who knows but this principle, this organ, may consist in the winding cylinders we have observed? But what can be expected from an insensible substance, and one on which poisons do not seem to have any action?

But before we proceed farther, we must see whether these canals or cylinders are found in other parts of the animal, and whether they form a system of vessels and organs, hitherto unknown.

We first observed them in the external cellular membrane of the nerves, tendons, and muscles. I have since shown how they may with ease be found

in all the cellular membranes of these organs ; so that their whole cellular substance is a web of winding canals.

I have observed that when the primitive nervous cylinder is covered with winding cylinders, its size is more than double what it is when it is stripped. A great number of these nervous cylinders form nerves, which are larger or smaller, and several of these nerves are usually united together, to form the very large ones. All these nerves have proper cellular coverings and common ones, and these coverings are made of winding cylinders. If I now suppose that the mass of nerves is formed of two parts of winding cylinders, and of only one part of primitive nervous cylinders, I do not think I am much mistaken. By applying the same mode of reasoning to the tendons and muscles, it will be found that the winding cylinders make up the greater part of these two substances, because the primitive tendinous and fleshy threads are of the same size with the winding cylinders, and these encrease their mass by the numberless windings they make round the primitive cylinders of these parts, over which they heap themselves.

Some hundreds of primitive threads, whether fleshy or tendinous, form the primitive *fascia* ; and a great number of these at length form the muscle or tendon ; so that I think I shall not be led into an error by the belief, that out of six parts, of which the muscles and tendons of an animal are composed, five of them are winding cylinders,
and

and one only the primitive threads of these two substances.

We already see that according to this, a great part of the animal solids is composed of winding cylinders; it remains to examine whether these cylinders are found in other parts. This research, which will throw a great light into the knowledge of anatomy, is very important.

First, it is natural to suppose, that if these winding cylinders are generally found in the cellular membrane of the nerves, muscles, and tendons, they ought likewise to be found in the cellular substance of other parts; and as the membranes themselves are no other than cellular, the winding cylinders should consequently be found in these membranes. It would be tedious to give a detail here of my observations on these parts; I reserve the doing this for another occasion. It will be sufficient to say that I have found the whole cellular substance to be formed of winding cylinders, in whatever part of the body it is met with. I have found them in the membranes of the brain, in the pleura, the peritoneum, the mesentery, the mediastinum, the pericardium, the periostium, and the pericranium, and in the ligaments of the liver and other viscera. The membranes of the arteries and veins are formed of these cylinders, and their internal coats which seem so compact, are woven with them. All the cellular substance, the sacculi, and the vesicles which contain the fat, are a web of these winding cylinders. In a word, I know of no part in the body which has a cellular membrane,

and does not present these winding cylinders. I should except the membranes of the vitreous and crystalline humours, in which I was not able to observe it; and also the lamina of the transparent cornea, which did not present them to me with certainty. If these membranes be destitute of winding cylinders, they must be distinguished from the ordinary cellular membrane, and are of another nature.

If it now be considered that the cellular substance is found in all the organs of the animal body, and that all the solids are principally composed of it, it will readily be concluded, that the winding cylinders form the greater part of these solids, and that all the rest is very trifling when compared with them.

The use of so great a number of cylinders is without doubt of the highest importance; but this is not the place to treat of it. The subject requires new observations, and very many experiments, the greater part of which I have not yet made. I have indeed thought that the matter of which they seem to be formed, is a glutinous substance similar in consistence and colour to a gelly or a mucous matter. I am somewhat apt to suspect, that the gelatinous matter which is drawn from the animal substance, is nothing else than the matter of which these winding cylinders are formed. But I repeat that I have not yet made a sufficient number of experiments to determine with certainty, either their real nature, or the uses to which they may be destined in the living body. It is enough for me at present to have established the existence, size, and extent of them.

REFLECTIONS ON THE MOTIONS OF THE MUSCLES.

THE primitive nervous cylinder is absolutely simple, is never found larger or smaller in its progress, and is not subdivided into lesser branches. It appears besides to be filled with an homogeneous, transparent, concrete, matter or fluid, which seems in different cases, to form itself into irregular spheroidal corpuscles, more or less elongated, and generally of a much smaller size than that of the red globules of the blood.

In animals, the nerves ramify much less than the arteries and veins, and their ramifications diminish in size much more than those of the blood-vessels ; observation has therefore established this truth, that there is a less number of ramifications of nerves, in any given part of an animal whatever, a muscle for example, than of veins and arteries : it follows

hence, that the space occupied by the blood vessels must be much greater than that occupied by the nerves.

The primitive nervous cylinder is about thrice as large as the primitive blood-vessel, and this last is about four times as large as the primitive fleshy thread. Thus is the primitive nervous cylinder about twelve times the size of the primitive fleshy thread, and when the primitive fleshy fasciæ are attentively examined, we can scarcely say that we observe blood-vessels in them, and remain uncertain whether we really see nerves in them or not; and it is absolutely impossible in any circumstance, to see either vessel or nerve proceeding to the primitive fleshy threads. If there were any blood-vessel or thread of a nerve betwixt the primitive fleshy threads, these threads would not meet in contact, as they actually do for their whole length; their distances would be rendered four times greater by the blood-vessels, and twelve times greater by the nervous cylinders. These would be much more visible to the microscope, on account of their size; but we observe nothing of them. The fleshy threads cling to each other in such a way for their whole length, leaving scarce any space betwixt, that we can scarcely perceive the very fine cellular web, or glutinous principle, that fastens them together.

We may I think deduce from hence, that a muscle is in the greatest part formed of primitive fleshy threads, and in the least of primitive nervous cylinders.

ders. The blood vessels preserve a medium betwixt these two quantities.

We may likewise reasonably deduce, that the primitive fleshy threads are not accompanied throughout, nor environed on all sides, by blood-vessels, and still less by nerves; and I am almost inclined to think, that an entire fleshy fascia, scarcely receives any primitive blood-vessel, or primitive nervous cylinder. Hence it is scarcely probable, not to say absolutely impossible, that every fleshy thread can receive, either a venous canal, or a primitive nervous cylinder. These seem to be the natural consequences of immediate observation, and of the very great disproportion there is betwixt the size of the primitive fleshy threads, and that of the minutest blood-vessels, and the primitive nervous cylinders.

It would be unaptly objected, that the primitive fleshy threads could not vegetate, if there were not blood-vessels to nourish them; that they could not contract, if there were not nerves throughout; and that they would not be sensible, if they were not accompanied throughout by primitive nervous canals. It is needless to remind, that to enable any part to vegetate or encrease, it is sufficient that a fit humour be conveyed to this part; and that a humour may reach a part, by other vessels than by those of the blood, or by simple transudation. The contraction of the muscles does not necessarily suppose that the nerves must penetrate into all the minutest part of a muscle, and that they ought to touch, surround, and envelope every one of its elementary

parts, which could not even be combined by the ordinary quantity of nerves. Sensibility is in the muscles in general, and no experiment can demonstrate that the primitive fleshy thread is sensible, and much less, that it is so in all its parts. Our senses are too blunt to reach so far as this.

The consequences we have just drawn from the primitive structure of the nerves and muscles, not only do not favour the different hypotheses naturalists have imagined to explain the contraction of the muscles; but likewise demonstrate that the greater part of them is absurd. What however seems very clear is, that this undulated structure of the primitive nervous cylinders, and of the fleshy threads and primitive tendons, serves admirably well to resist very violent efforts in the machine; these parts being able to bear a very great extension without breaking.

An important question now presents itself, which is precisely occasioned by the very structure of the primitive nervous cylinders. It is, to know whether the nerves are irritable; that is to say, whether they contract when stimulated by any substance, or when any of the muscles are contracted at will. All the observations and experiments hitherto made by the best naturalists, assure us that the nerves are not acted on by any stimulus, that is to say, that they do not diminish in length, nor alter their size, whether they are externally pricked, or whether the animal employs them in contracting the muscles: but these experiments prove nothing, unless it be that the external coat of the nerves is in these cases altogether useless; they do not prove that the internal
parts

parts of these same nerves is motionless, and that the primitive nervous cylinders are not capable of shortening themselves. The external coat of the nerves is not so formed as in any way to prevent an oscillatory or contracting motion of their primitive canals, and the winding structure or progress of these cylinders would lead one to suspect the contrary.

But it must be left to experiment to decide on all occasions when it can be consulted; our reasonings very seldom exceed the limits of simple conjecture, even when they seem founded on the completest analogy.

If the primitive nervous cylinders change their situation whenever the nerve is stimulated, the spiral figure which results from the position of these cylinders, ought necessarily to be more or less changed, and the spaces betwixt one spire and another, or betwixt one band and another, must become greater or smaller: it is at least certain, that the progress of the primitive nervous cylinders in a nerve cannot be deranged, without the spires being deranged likewise. A common lens enables one to see the spires of the greater part of the nerves very well; thus is the observation easy, and the preparation of the parts for the experiment not at all difficult. I principally made my observations on the nerves of the diaphragm of small animals, such as rabbits, and on the crural nerves of frogs. As soon as I had opened the animal, I employed some one to stimulate the phrenick nerves
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in the thorax of the rabbits, and the crural nerves at their going out of the vertebræ. The diaphragm, and likewise the legs and feet of the frogs contracted themselves. At this time I observed the spires of the nerves with such attention, that the smallest change in them could not escape me. I examined the nerves of the diaphragm at their minutest ramifications, where the spires are the most simple. I repeatedly examined the small nerves in frogs, that are sent to the abdominal muscles, and in which the spires are yet more visible. But whatever diligence and attention I bestowed on them, I never could perceive any alteration in their spires. However violently I stimulated the nerves, and however great the contraction of the muscles, I invariably found them motionless, and at the same distance from each other as before.

Having lost the hope I had conceived, of seeing some motion in the spires of the nerves, I proceeded to the last experiment I had to make on these organs, which was to examine in an immediate way, their primitive nervous cylinders themselves. This observation cost me a great deal of pains, and I cannot flatter myself with having so nicely observed the absolute immobility of these threads, as I did that of the spires. But what I observed in them did not indicate motion, and as they have always appeared to me at rest, I shall invariably believe, till some one shall be more successful than myself, that these primitive nervous cylinders are in that state, when the nerves are stimulated, and the mus-

cles contract ; and I shall believe it the more readily, as I cannot conceive how the cylinders can undergo a sensible change and vary their situation, whilst the spires or bands remain quite motionless.

This singular structure of the nerves, and the regular progress of the primitive nervous cylinders, which seemed to promise some fresh discovery as to muscular motion, and some new use of the nerves themselves, leave us as much in the dark as before, and only serve to diminish the number of hypotheses that have been imagined to explain the contraction of the muscles : so true is it that before we can attain a knowledge of the truth, we must pass through all the errors that surround it. The system of the vibrations of nervous threads, for example, seems no longer capable of being supported, after my observations. I speak of those vibrations that are capable of being perceived by the microscope, and I give up to metaphysicians the privilege of inventing invisible ones.

The pretended very great velocity of the nervous fluid seems to be contradicted by that inert, viscous fluid or matter with which the primitive nervous cylinders appear filled. The considerable size of the nervous cylinders and blood-vessels, when compared with the primitive fleshy threads, leads me to suspect that these threads are not put in motion, in an immediate way however, either by the blood, or by the nerves. In a word, we are not only ignorant of muscular motion, but we cannot even imagine
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any thing to explain it, and we shall apparently be driven to have recourse to some other principle ; that principle, if it be not common electricity, may be something however very analogous to it. The electrical gymnotus and the torpedo, if they do not render the thing probable, make it at least possible, and this principle may be believed to follow the most common laws of electricity. It may likewise be more modified in the nerves than in the torpedo and gymnotus. The nerves should be the organs destined to conduct this fluid, and perhaps also to excite it : but here every thing yet remains to be done. We must first assure ourselves by certain experiments, whether there is really an electrical principle in the contracting muscles ; we must determine the laws that this fluid observes in the animal body ; and after all it will yet remain to be known what it is that excites this principle, and how it is excited. How many things are left in an uncertain state, to posterity !

*Microscopical Errors ; and Consequences deduced from
Microscopical Observations.*

Such have been my observations on the structure of the nerves, tendons, and cellular membrane, and my conjectures on some of the uses of these parts. I have so contrived as to give, to my principal observations at least, all possible certainty. I have endeavoured

deavoured to render them as simple as possible, and have established them by direct and diversified experiments.

There is a great difference betwixt a microscopical observation, and a microscopical experiment. The first is no other than a simple representation of an object, in the circumstances in which we observe it ; the second is the analysis of the representation of this object, by which we are assured that an object of such a nature, and not of any other, does actually exist. In the first case, we simply feel an impression of the light, or view at most nothing more than an image ; in the second, we judge of the nature of an object by the image it presents to us. Every one is capable of distinguishing with the microscope ; but few know how to judge of the things they see. Extensive knowledge, and the greatest sagacity, are requisite to an observer, to conceive the experiments that necessarily conduct to a true acquaintance with a real object.

There is a species of error into which even the most practised observers may easily fall. All our opinions on bodies are no more than simple comparisons ; that is to say, we judge that such a body, seen with the microscope, is of such a nature, and not of any other, because it presents itself to us under the same forms or appearances, under which we are accustomed to see some object, when we examine it with a microscope, that is already known.

It is morally impossible that two images should be every way alike, and that the external objects
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they represent should be every way different. An observer of penetration, if two objects are not similar, will at length find some difference in the appearance of them, and will avoid an error. But how much industry and attention must he not necessarily employ ? This however is not the most dangerous shoal, although very enlightened observers sometimes run aground upon it. It sometimes happens that we see a certain object with a microscope, which seems to agree in every respect with the bodies we are best acquainted with, particularly when the structure of these bodies is not very common. It then seems impossible to us that an image can have so singular and perfect an agreement with objects that we know, and that the object which furnishes it can be notwithstanding of a perfectly different nature ; it is not impossible however but that this may happen. When, afterwards, the images that are represented are irregular, and these irregularities are so many consequences resulting from the object which we figure to ourselves to exist, we have not a moment's doubt, and yet may still be deceived.

To prevent the springing up of these errors, it is absolutely necessary to analyse the observation itself ; that is to say, to make a microscopical experiment capable of assuring us, that the object is in reality such as the microscope has figured it to us. This is the most difficult part of the undertaking, and this is what makes a difference betwixt observation

vation and observation, betwixt observer and observer.

A simple bare observation does not admit a full confidence, even although it be made by a celebrated observer, because it tacitly supposes that there is a necessary and exclusive agreement betwixt the image represented by the microscope, and the real exterior object ; which is not always the case. The image represented by the microscope may agree with several objects at the same time, and it belongs to the industrious observer to determine which is the real exterior object it represents and corresponds with. It is then not sufficient to say, I have seen so and so. The circumstances must be determined, and the observation varied a thousand different ways ; it must in short be analysed, and the experiments rendered decisive.

Of all microscopical observations, I know of no one that can easier lead the most consummate and penetrating observer into an error, than the external structure of the nerves, in all of which it seems impossible for us not to distinguish, and that very constantly, a spiral form of the utmost regularity. The more we observe them, whether they be large or small, simple or composed ; whether we examine their principal trunks, or their least visible branches ; we remain more and more persuaded of their having a constant spiral structure. Their very irregularities serve as proofs that they are all of this form, rather than they tend to deceive us.

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They not only conciliate themselves to this structure, but beome so many luminous consequences of it.

The structure of the nerves I have examined, tends greatly to a doubt, even of those observations that appear the most constant and certain, and should at once inspire us with moderation and mistrust. A haughty and confident tone only becomes an ordinary observer, quite ignorant of the innumerable circumstances which may deceive him, and is frequently the character of a visionary, who is more eloquent than judicious; of a learned man who loves rather to divine nature in his study, than to consult her where she is; or rather, of one who is fond of substituting dreams and visions, to truths and matters of fact. A single experiment, one lonely observation, has often reduced to vapour, whole libraries of such philosophical romances; and to the shame of our age be it said, they are still multiplied, and persons are found who are fond of losing their time in reading them.

No part of what I have just now observed can be applied to the celebrated Dr. Monro of Edinburgh: He has not sought to divine the nature of the nerves, but has examined into it; neither has he invented vain hypotheses, but has consulted nature herself. If he has been notwithstanding mistaken, it can only be observed, that error is more accessible than truth. Truth is simple, error is infinite; or rather, if truth expresses itself by unity, error is capable of expressing itself by infinity, or

at least in so extensive a way as nearly to approach it. It is certain, in relation to us, that error assumes all the appearances of truth, and may seduce us a thousand different ways. To be satisfied of this, it will be sufficient to read the history of human errors. The greatest philosophers have not been exempt from them, and the most enlightened nations have fallen into errors that have continued as long as themselves.

The efforts then that man makes to come at the truth, are a kind of game of hazard, in which the probability of falling into an error is very great, and that of attaining at a truth very small. Those who arrive the latest are least subject to err, since they profit by the errors of others, by finding the number of cases that lead to them diminished. Hence arises the impossibility of a work, complete, and original in all its parts, being produced by one man, in a single age.

I hope the enlightened reader will pardon me this small digression on microscopical errors, and that he will look upon it as a kind of apology for my work itself, which, whatever care I have taken to bestow the attention of which I am capable, and which my circumstances have permitted me, on my observations, I do not think free from human imperfections. And where is the observer who can ever assure himself of having seen every thing, and if I may so express myself, of having exhausted nature, in the first trials he may have made, on so many small and distinct parts of the animal body!

After a century of observations made on the blood by so many good observers, from Lewenhoeck down to Father della Torre, we still seem ignorant of the figure, composition, size, and structure of the corpuscles that give a red colour to this fluid: this will be seen in my *microscopical observations*, when I determine, imperfect as they are, to make them publick. It will now be sufficient to excite the attention of naturalists on these subjects, and to give a necessary impulse to the most skilful observers, to the end that they may bestow their industry on a subject so important to the understanding the mechanism of the animal machine. When I shall myself return to these matters, with a mind tranquillized and refreshed, I may add many important things, that may have escaped me the first time; and may correct others which I now think have been well observed. The true merit of a naturalist is in proportion to the number of discoveries he has made, and to the difficulty and importance of them. Man is liable to errors, and they should therefore be pardoned him. They must be considered, to adopt the language of geometricians, as *quantitates evanescentes*, in comparison to the discoveries; when these last are many, useful, and original. *Dices enim*, (says the great Haller) *cum fide autorem esse, cum quo naturam sæpius consentire videbis, &c.* But when these discoveries are not found in a book, error entirely discredits the work, and its authour. The least blameable is the man who copies truths already published,

published, without adding errors to them; but his name will not be handed to posterity, because it will be indebted to him for nothing new, which may serve to recall the recollection of him.

Nothing but ignorance and envy can possibly confound the merits of these two classes of writers, and unjustly attribute to one, what the other alone is deserving of !

Observations on the Hairs, Epidermis, Nails, Bones, and Fat.

The learned Edinburgh professor, after having examined the parts of animals that are most composed and most interesting, those on which we have remarked above, extends his observations to many other parts, less important if you please, but which on that account are not better known : such as the nails, hairs, epidermis, &c.

He finds nervous spires likewise in these parts, or pretends rather that they are composed of nervous cylinders, although he agrees besides, that their nervous substance is not sensible; that is to say, that the nerves woven in them are not organs of sensation, as they are in general in all the other parts of the animal body. The opinion of Dr. Monro is by no means absurd, although it may be false on the side of observation; because it is certain that many of the parts which are sensible, and

provided with nerves, harden, ossify, and lose their sensibility. But does observation clearly demonstrate that all these parts have been composed of nerves?

Dr. Monro assures us that these organical parts are woven with cylinders entirely similar to those of which he believes the nerves to be composed. This resemblance can only have a relation to the size and respective figures of the part, so that the consequence he wishes to deduce from it, is neither a demonstrated, nor an established, truth. But though it should even be proved that all these parts are composed of winding cylinders, in every respect like those that surround the primitive nervous cylinder, it does not on that account follow, that they must be composed of nerves, nor that they must necessarily accompany the nerves in these parts; since we have seen above that the cylinders which serpentine and run along the nerves, although they form the coats of nerves, are not so in themselves. The cellular substance in the body is quite woven with these cylinders, and is nevertheless not composed of nerves.

But this does not render the Doctor's observation unworthy attention. If all the parts in the body had the same structure, if they were all composed of winding cylinders, and if these cylinders were in all of them of the same size, the knowing that there is a general primitive texture common to all the solid parts of the body, would however be an excellent discovery.

Let me examine then the reality of this particular, and in the course of my observations, pay as scrupulous an attention as I can to these small bodies, which can scarcely be distinguished with the strongest glasses.

My first observations relate to hairs. I shall only mention a few of the principal of them; as many as are necessary to give a general idea. I took a hair, and cleansed it several times by passing it slightly across a piece of fine linen moistened with water; I then examined it with lens' of different strengths, employing some that magnified 400 times, and so upwards to 700, and have invariably seen the same structure, and the same composition. Fig. 1. Plate I. represents an end of an hair, in which, towards its axis at *a, a*, I observed a dark spot, which was interrupted towards the middle of its length. All the rest of the hair was of the colour of transparent amber. It appeared to be woven, and formed, or covered, with small interrupted cylinders, winding in the manner of intestines. Among these cylinders, there were here and there very small globules, the diameters of which did not exceed those of the cylinders themselves, and these globules were in several places situated betwixt' the intervals of them. The figure mentioned above represents all this very clearly.

I bruised the hair *m, m*, towards one of its extremities, and it seemed to me to be formed of several irregular rugged trunks, composed of groups, or bundles, of very small winding cylinders. Fig.

2. represents one of these trunks, which I bruised more than I had done before. I found it as it is seen in the figure, that is to say, formed of winding cylinders, with many globules or round bodies, dispersed over them.

Fig. 3. represents a small portion of the hair of Fig. 2. separated with the point of a needle; in it are seen several globules detached from the winding cylinders.

Fig. 4. shows another small portion of the hair of Fig. 2. which is pretty much like the other. However when I looked at it after I had wetted it well with water, it was become like an irregular transparent pellicle, in which there scarcely remained any traces of globules or winding cylinders; but after I had left it to dry, it soon recovered its first shape, appearing as it does in Fig. 4.

On the Transpiration.

The learned Father della Torre, who has busied himself so much in observing the minutest parts of the animal body with very strong lens', assures us of his having discovered that we transpire through the epidermis, an immense quantity of small, transparent, differently shaped lamina. These lamina heaped some over the others, compose the scales of the cuticle, which he says is interwoven with lymphatick vessels. Lastly, he adds, that
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these small laminæ are even seen with the naked eye, in the viscous humour that covers the skin of eels.

In imitation of Father della Torre, I washed one of my fingers well in several waters, and having repeatedly dried it, applied it to a glass, keeping it there for some time, till the glass became soiled with it. On making use of a strong lens, which magnified upwards of 700 times, I could distinguish nothing besides very small transparent globules, which neither dried nor disappeared, as aqueous vapours do, but continued the same on the glass. Although there were several that touched each other, they did not unite their substances, to form larger ones. They were all of the same size and equally round, as is seen in Fig. 6. which induces me to believe that they are not a pure aqueous vapour, but rather a gross oily substance. They are about four times smaller in diameter than the red globules of blood. Fig. 7. represents the size of a globule of blood, seen with the same lens as the globules of Fig. 6.

I repeated these experiments a great many times; on the transpiration of the epidermis of many other parts, and could only observe the globules of which I have spoken.

On the Gluten of Eels.

Being desirous of examining the gluten of the skin of eels, I had several brought me of different sizes,

and found, after taking the gluten in a very small quantity, and diluting it a little in water, that it was formed of uniform irregular bladders, filled with very minute spheriodal corpuscles, as they are described by Fig. 8.

I suffered them to dry on the glass, and they then appeared as they are seen in Fig. 9, that is to say, more irregular than before, with large transparent edges all round them, with irregular corpuscles scattered in different parts within.

I broke two or three of these bladders or vesicles, and a great number of very small corpuscles poured from them, as is seen in Fig. 11.

The vesicle *a*, of Fig. 10, represents one of the vesicles of Fig. 9, in which an oviform body is observed, with a spot in its middle. At its side is placed a globule of blood, *c*, by way of comparing their relative sizes.

On the Epidermis.

After having repeatedly washed one of my fingers with great care, I detached from the epidermis several very fine laminae, which had scarcely a sensible thickness, with a razor. Fig. 12, of Plate VIII. represents one of these fragments examined with a very strong lens. It appeared to be a web of winding cylinders which approached each other, and retreated, with great order and regularity: very small globules were here and there distinguished in it. I now
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covered one of these laminæ with water, in which state it appeared more transparent, and the cylinders and globules were seen more distinctly than before. I employed glasses which magnified 700 times in diameter, but could perceive nothing more. Neither hole nor porosity could be distinguished with the microscope, so that I can venture to say, that those who believe they have seen pores in the epidermis, are absolutely deceived. Lewenhoeck was the first to lead other observers into this error. Not that I wish to assert that there really are none, I only say that they are not observed with the microscope. It is very probable that the lymphatick vessels which Father della Torre says he observed in the epidermis, in the form of net-work, are no other than my winding cylinders; and I think them badly described in Fig. 7, of Plate XII. of his work, and that they do not form a net-work, as he wishes to believe.

On the Nails.

I detached a very fine lamina from one of my nails, with a razor, and on examining it with very strong lens', found it to be formed of the usual winding cylinders. They appeared here however a little closer than usual, and perhaps somewhat smaller. I found the usual globules scattered amongst them. Fig. 14. represents a small portion of a nail, which I examined. I then covered another portion with

with water, and observed it in this state. It seemed to me that the winding cylinders were diminished in number, and were in some measure straightened. Every thing appeared more transparent and homogeneous, as is represented by Fig. 15.

On the Bones and Teeth.

The very bones, and the enamel of the teeth itself, are composed of the usual winding cylinders. I have not thought it necessary to describe them particularly by figures. The only difference I observed was, that the winding cylinders which compose the enamel of the teeth, are shorter and better united together.

On the Fat.

I wished likewise to examine the fat, and observed that of several animals in its natural state. I found betwixt the laminae of the cellular membrane an infinite number of small bags or bladders, which differed in size in different animals. These vesicles were filled with fat, or with an oily humour, either more or less concrete, or quite liquid. The vesicles were heaped some over the others, and covered and surrounded on all sides with winding cylinders, as is seen in Fig. 19, of Plate VIII. By the means of warm water, and a few pricks of a needle, I con-

trived to separate from these vesicles several of the winding cylinders, and I then perceived a bladder formed by a very subtile, transparent, homogeneous, humour, filled with fat, as at Fig. 20. I could not observe any thing fastened to it, nor any excretory or secretory vessels, whatever attention I paid to it. I compressed some of these vesicles belonging to fishes, filled with oil, and observed in these cases, that the oily substance transudes on all sides, and regularly, through the sides of the vesicle.

The fat of different animals, as well with warm blood as with cold, which I examined, appears to me to be seated in round bags or bladders, larger or smaller, and more or less regularly shaped, situated in the cavities which the laminæ of the cellular membrane leave betwixt them. I will not deny but that a quantity of the fat, not enclosed in the bags, but loose, may be found betwixt these laminæ. I only mention what I have observed repeatedly in several animals. This observation has frequently surprized me, because I knew it to be quite contrary to the received opinion. Thus I cannot determine whether the vesicles of the fat are naturally destitute of particular conduits intended for receiving it, and transmitting it elsewhere, according to particular circumstances and exigencies; I only remark, that although I have sought them very attentively, I have not yet been able to see them. If it be true that these conduits do not exist, the vesicles must be supposed to absorb and reject this oily matter, by means of porosities in the pellicles. This seems

confirmed by the experiment on transfusion I have related above.

On Ivory.

I separated a fine scale of Ivory, which had been previously well polished, with a sharp knife. I observed it in water in a refracted light, and in the same light I viewed it dry. In the last instance it seemed to me far less transparent than in the water, but equally organized. Fig. 21, of Plate VIII. represents this scale or lamina, in which the usual winding threads are distinguished.

On Sponges.

Curiosity led me to examine with some attention, the substance of which sponges are formed. They are believed to be the work of animals. Fig. 1, of Plate X. represents a branch of this substance. I viewed it in an obscure, and in a refracted light. It seemed likewise provided with the usual winding threads, and was hollow in the middle.

This is all I have been hitherto able to observe of the structure of the animal body. I confess that a great deal remains to be done, and am not altogether satisfied with the observations I have made. I flatter myself that at a future time I may be able to clear
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up many doubts that still remain. The first advances are however made, and these are always the most painful; when the passage is once cleared, it is no longer so difficult to perceive the course we ought to take, and to avoid the precipices that may lie in our way.

It seems to me very likely that the primitive cylinders are absolutely no other than what Dr. Monro calls nerves, and which he believes to be truly so. He finds them indeed of $\frac{1}{9000}$ of an inch in thickness, whilst they appeared to me to be of $\frac{1}{13000}$. But as this relates to a very minute body, it is not impossible but that the same object may be judged $\frac{1}{3}$ larger or smaller by two different observers. Father della Torre, and Jacques Jurin, differ from each other, as twenty-seven to one, in determining the size of the globules of blood.

The mistake of the celebrated professor of Edinburgh seems to me to consist in his having taken these winding cylinders for the nerves themselves, whilst they are only the coats or sheaths of them. They are common to the muscles, tendons, and viscera; and lastly, to the cellular membrane, with which all the organical parts of animals are woven or enveloped. They are distinguished in the epidermis, in hairs, in the nails, in the bones, and in the vesicles of the fat; they are almost uniform every where, both in their progress and size, so that I find no difficulty in believing that they are of the same nature and quality in all the parts of a living body, and that they tend to the same ends,
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and have the same uses ; it must however be observed, that they may harden more or less, according to accidental circumstances, above all in the different organs; and different states, of the animal machine.

On Vegetable Substances.

Dr. Monro is not content with examining all the parts of the body, but carries his researches to vegetable substances themselves. He finds them to be formed of winding cylinders, exactly similar to those he observed in animals, and of which he supposed the nerves to be composed.

I shall relate in a few words some of the very many observations I made on plants ; and think they will be sufficient to determine us in what we ought to credit on the subject.

When in London, I examined the famous moving plant called *hedyfarum movens*, and sought the interior mechanism of that regular perpetual motion which distinguishes it from all other plants, and brings its nature so near to that of animals. Although I found nothing satisfactory as to the object of my curiosity, I did not meet with any difficulty in discovering canals or cylinders, and that principally in the stalks of the leaves, which seemed to be composed of spires or bands, as they are described in Fig. 13. of Plate X. But I
soon

soon perceived these to be vessels formed of a single thread, which turned round a common centre, and, closing on all sides, formed the coat of the large vessel itself. I succeeded in unfolding great lengths of these, and then knew them to be the pipes (trachées) of plants, known before my time. The spires or bands are here real, instead of which they are in the nerves only apparent, although in both they appear perfectly alike: so true is it that analogy is very subject to lead us into errors.

Fig. 14. of Plate X. represents the pipe partly unfolded. The threads of which it is formed are transparent in their length, and their edges are obscure in such a way, that by meeting each other they form dark bands.

There is however in plants, another structure of parts; a more general organization. This organization seems to compose the greater proportion of their mass, and is perfectly analogous to the winding cylinders we have observed in so many parts of the animal machine.

Fig. 15. of Plate X. represents a small fragment of a rose leaf, partly stripped and torn, with the point of a knife. The usual globules and winding threads are observed in it.

Fig. 2. of Plate X. represents a very small bit of elastick resin, in which are seen the winding threads.

Fig. 22. of Plate VIII. represents a cotton thread observed dry.

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The surface of the cotton thread seems interspersed as usual with small cylinders. Fig. 23. of the same plate shows two of these cotton threads put in water: there are fewer of these winding cylinders on their surface, they are less regular, and seem to penetrate into the inner part of the threads.

Fig. 12. of Plate X. is a fragment of amber, which, like all other bodies, seems covered with winding cylinders.

On Fossils.

Dr. Monro's observations on fossils are very singular, and have the air of a true paradox. He believes them to be all formed of winding cylinders. Earths, salts, metals, he finds them all made of cylinders, and even discovers them in gold coin; in guineas. He brings nothing but observations to support his opinion, and observations ought to be combated or confirmed by observations, and not by words.

I examined several of these substances, and shall only mention a few of them, intending to treat of them more at length in my *microscopical observations*, when I shall give my sentiments on this obscure matter, which I presume will long divide the opinions of observers. Nothing less than the seeing the smallest constituent parts of bodies is necessary to know whether there is an organical, simple,

sole, primitive structure, common to animals, to vegetables, and to fossils. Here simple observation is not sufficient, and it does not avail much to be skilled in the microscope. The apparent object does not distinguish itself from the real one. Not to be deceived, observations must be analyzed, and experiments made; but the road in both is long and painful, and we do not always see how we are to fall into it.

I began my observations by examining earths and marbles. Fig. 7, of Plate IX. represents several grains of calcined magnesia, which I observed both wet and dry in a refracted light. They were in a greater or less degree of a cylindrical shape, and rounded at their extremities. The usual winding threads were likewise very distinctly observed in them. When in water every thing was very transparent.

Fig. 9, of Plate IX. represents two atoms of white marble observed dry. The usual winding threads were likewise seen here.

Fig. 10, of Plate IX. represents an atom of heavy spar, which I observed in a refracted light. Nothing was discernible besides the winding threads; which were very regular.

Fig. 11, of Plate IX. represents an atom of phosphorick spar, which displays in every part of it the usual winding threads.

Fig. 3, of Plate X. represents an atom of common salt. The winding threads are found in it, but not so abundantly as elsewhere.

On Gold.

From earths I proceeded to metals. I drew out with the wire-drawing iron, a cylinder of very pure gold, and after having scraped and polished it well, I observed it breadthwise. It appeared quite covered with winding threads, as is described in Fig. 1, of Plate IX. Fig. 2, of the same Plate, is the same cylinder observed longitudinally. I examined it both ways in a reflected light.

Fig. 3, of Plate IX. is a small piece of gold leaf. It appeared to be woven with simple winding threads. I viewed it both wet and dry.

From gold I proceeded to the examination of silver. The four small objects of Fig. 4. Plate IX. are atoms of very pure silver filings. They very much resembled hairs. In a reflected light the usual winding threads were seen in them; but in a refracted light all was obscure, except at the very extremities, where the winding threads likewise showed themselves.

I examined a very thin plate of lead in a reflected light, and observed the usual winding threads, as they are seen in Fig. 10, of Plate X.

Brass, likewise, in a reflected light, discovered these winding threads, as they are represented by Fig. 11, of Plate X.

Tin, observed in a reflected light, likewise showed these

these winding threads. Fig. 7, of Plate X. is a small fragment of tin observed in this way.

Antimony, as is seen in Fig. 8, of Plate X. has these winding threads. It was observed dry, and in a refracted light.

Fig. 9, of Plate X. represents an atom of cobalt, in which are observed the winding threads.

Fig. 6, of Plate IX. represents an atom of zinc, observed dry, and in a reflected light ; it has winding threads as usual.

Fig. 8, represents an atom of bismuth observed dry in a reflected light ; the usual threads are distinguished in it.

Fig. 12, shows an atom of nickel, which, observed wet, had the winding threads.

I pass over in silence a great many other observations on fossils, which all agree with those we have hitherto seen, so that it seems a truth established by observation, that all bodies, viewed with very strong lens', display the same form or appearance.

This is not the place to determine what is real, and what is only apparent. Simple observations are not sufficient to decide with certainty. There is need of a very nice analysis of all the circumstances ; it is necessary to prepare the substances for observation ; in a word, experiments are wanted. I wish to avoid the giving my opinion on this matter. I cannot do it with the brevity that is requisite, and it will be the subject of another work. In the mean time I shall be very happy to know the

different opinions of observers on this subject. The most skilful will be the last to determine; those who are pretty much so will not find any great difficulty; and the novices, and all those who do not observe at all, will decide at once.

To retard a little at least, the judgements of all of them, I think it proper to subjoin as briefly as I can, two important particulars. I let fall into a basin of cold water some drops of melted silver. I afterwards examined several of the smallest of them, and instead of finding the usual winding threads, they appeared to me to be formed of small shining grains, embossed at several parts of them. Fig. 4. Plate X. represents a corpuscle of silver in which some of the winding threads are however seen scattered. The other parts seemed formed of unequal globules. I viewed it in a reflected light.

Fig. 5. of Plate X. represents another small bit of silver in which the very small shining corpuscles are seen, without any winding threads. I examined it in a reflected light.

Fig. 6. of Plate X. is another small portion of silver, examined in a refracted light. It seemed formed of points, pyramids, and very small diamonds, and I could observe no winding threads in it. These different structures in the same body engaged me to try a few fresh experiments.

I scratched, with the point of a needle, a small bit of talc, which was transparent and homogeneous throughout. I examined it with the lens, and it appeared to me in the way it is described in Fig. 16.
of

of Plate VIII. There was a ridge in the middle of it, with winding threads, and globules: The threads did not vary much from those I had observed before. I wet it with water, and polished it with linen; but it still remained in the same state.

Upon this I suspected, that probably the simple contact of very minute round bodies was capable of causing this appearance of winding threads. In consequence of this suspicion, I examined dry hair-powder, but could observe nothing in support of it. Although the globules touched each other in several parts, they were very distinct from each other, as is represented in Fig. 18. but scarcely had I wet them a little, when I observed in some places, continued, longitudinal, homogeneous, bodies, transparent throughout, as appears in Fig. 17. of Plate VIII. It is true that these winding threads were very few in number (*a*), when compared with the globules which surrounded them, and that they were scattered here and there without that regularity and parallelism which is so constantly observed in other bodies. But we learn at the same time by this observation, and by this experiment, that water is capable of so insinuating itself and settling betwixt the globules, that in certain instances it imitates an homogeneous, transparent, uninterrupted, thread, or cylinder.

I repeat, that good observers will be very slow in pronouncing any thing in a decided way; but it is

(*a*) More of these threads are expressed in the engraving of the above figure, than what I really observed.

proper at the same time that observations should be made and varied in all possible manners. Observations alone may supply us with certain intelligences, after they have been well analyzed, and all the circumstances of them well known.

A Letter

A Letter addressed to Mr. ADOLPHUS MURRAY, a celebrated Professor of Anatomy at Upsal, in the Year 1778.

I ENCLOSE you three designs marked 1, 2, 3, (a), which I thought sufficient to bring to your recollection the idea of the new canal I have discovered in the eye, and which I had the pleasure to show you when you passed through Florence. 'Twas on this occasion that I had the satisfaction of knowing you, and of being favoured with your agreeable friendship.

To so enlightened an anatomist as yourself, it will be sufficient to simply point out the principal parts; every thing else would be quite superfluous.

The three figures 8, 9, 10, (Plate VII.) express the three principal sections in the eye of an ox,

(a) These are the three numbers annexed to the drawing sent to Mr. Murray; but to preserve the order of the plates in this work, I have employed in their place, Figures 8, 9, 10.

which I showed you when you was at Florence. I shall not mention to you the other sections, since I consider these three as the most essential and necessary to satisfy your enquiries.

Fig. 8. as you see, represents almost half the eye, observed in its inner part. Letter *n*, points out the tunica sclerotis; *m*, the ciliary body, likewise called ligamentum ciliare; *e*, the processus ciliares; *c*, the uvea; *a*, the pupil. You see by the design, that my canal of the eye answers to the circular band indicated by *m*, which forms the corpus ciliaris.

Fig. 9. is the half of the preceding figure. Letter *n* indicates the tunica sclerotis; *e* the processus ciliares; *c* the uvea; *a* the pupil. Letter *m*, at the the right of the same figure, expresses the corpus ciliaris divided; the opening points out the hollow of the body, or canal rather, I have discovered in the eye.

The three letters, *r*, *m*, *o*, not only show the canal, but likewise the upper side of it divided in two. Letter *m*, is the body of this new canal opened, and *r*, *o*, are the two extremities or lips of the incision made in the upper part of its coat.

You must perceive hence, that this new canal is formed of the ligamentum ciliare, which is wrapped in its substance; you will understand this still better by throwing your eye over Fig. 10. which is the other half of Fig. 9.

Letter *a*, in this figure, shows the tunica sclerotis stripped of the tunica choroides. Letter *c*, indicates

cates the ridge in which the ciliary ligament is fastened by means of the cellular filaments. Letter *r*, points out the transparent cornea. The three letters, *e*, *o*, *s*, belong to a membranous substance formed by the meeting of the tunica choroides, *e*, the ligamentum ciliare *o*, and the uvea *s*. Letter *o*, shows that part of the ligament which is inserted into the ridge *c*, of the tunica sclerôtis.

A very small crevice is discovered in the side of this canal at *o*; this part, or this side, of the canal is white and cellular, and is fastened very strongly to the tunica sclerotis through the whole of the circular ridge *m*, of Fig. 8.

I have passed water, quicksilver, &c. from one side to the other of this new canal, without the smallest laceration being caused by the passage of these fluids. Its coat within is very smooth and equal. The part *o*, can very easily be detached from the tunica sclerotis *e*, even by forcing it a little with a small scale of ivory; and the detached membrane, without being at all torn, is then seen to form the new canal as it is observed at *o*.

I send you the drawing of this new canal of the eye, not because I wish you, as you tell me you are desirous of doing, to publish it in the acts of the academy at Upsal, but simply because you ask it of me. It is enough for me that I am persuaded of my great esteem for you, and of the pleasure I receive from the correspondence of a man of your merit. Do what you will with it, for I am totally indifferent. You must certainly have perceived when you was
here

here how little I valued this discovery, now grown out of date with me ; I say *discovery*, since you will have it called so.

I shall however say nothing to you on the use of this new canal, and of the transparent liquor with which it is found moistened. I have at present no observation sufficiently sure, nor experiment sufficiently decisive, to render me certain thereupon. I will not advance imaginary hypotheses, nor simple probabilities. I abandon and submit this difficult subject to your genius, leaving to you the glory of clearing it up.

S U P P L E M E N T.

A D V E R T I S E M E N T

O F T H E

F R E N C H E D I T O R.

THE impresson of the two volumes of this work was quite finished, when I heard that our authour, always indefatigable, and always mistrusting his own labours, had, in the short intervals of leisure, which his painful and multiplied occupations afforded him, made a great number of experiments relative to the different matters he has treated of in the present work. He has very freely consented to the desire I testified to him of making them publick, by inserting them here under the form of a supplement. He has likewise communicated to me an extract, or rather, the dernier consequences, and

and most general conclusions, of a treatise on *opium* he has just finished, and which, if it were published entire, would alone form a work apart. It is a real pleasure to me to be able to enrich this edition with so many new and interesting truths, which together concur to render it more complete, and to give to so extensive a subject-matter, a perfection which would be in vain sought in the works of the most renowned naturalists of the two last centuries.

The learned and impartial reader will readily agree with me, that this work cannot fail of forming an epoch in experimental philosophy; and I care but little what the ignorant, and above all the envious, the number of whom is however but too considerable, may think of it. I shall reply to these, with the Latin poet, *Odi prophanum vulgus et arceo*.

Although I have inserted this supplement at the end of the second volume, the reader is begged to read it before the article that treats of "the toxicodendron, and some other vegetable substances;" or rather, to read the different parts of it jointly with the chapters to which they relate. The author has contrived it in this form, for the sake of brevity.

He has discovered an important truth, by rendering himself certain that the venom of the viper is a poison, when simply swallowed by animals; and that, contrary to the opinions of the best authors till this day, it kills in this way very suddenly.

The

The oil of the cherry-laurel, and the rectified spirit of this plant, which when injected into the veins of animals, instantly kill them, furnish another truth which removes the mystery and embarrassment as to the action of this poison, that had obliged our authour to make an exception to the common laws he had established, of the other poisons.

But what deserves the greatest applause is, his having found, when there did not seem the smallest hope or probability of it, having found, I say; a matter, which when united with the venom of the viper, renders it innocent; a matter, which may now be regarded as the true specifick against this formidable poison. This important and unexpected discovery, which we owe entirely to the indefatigable genius of our authour, has been managed with an extent and depth of investigation that are proper to him; and with that delicate analysis of experiments, which forces nature to unveil her most hidden secrets. Posterity will judge of the remedy, and of the merit of this discovery. In the mean time, I exhort naturalists to follow his steps in the road now open to their researches; to multiply experiments on the largest kinds of animals, according to the wish of the authour of this new remedy himself; and to determine the circumstances with the nicest exactness, in which it may be most useful and most certain. The remedy seems sure if given in time, and the specifick is discovered; it remains to establish the mode of applying

it with the greatest success, and the least pain and inconvenience.

The authour ends his supplement by setting in the clearest light, the action of opium applied to the different parts of a living body ; he proves demonstratively, that the proper vehicle for opium is no other than the blood ; that opium acts instantly on the blood ; and that in whatever way it is applied to the nerves, it produces no changes in them : three experimental truths of the greatest importance, and which oblige the reasoning philosopher to form a new theory on this subject, since they must cause almost all that has been said on opium, by the majority of other writers, to be regarded as errors and simple imaginations.

S U P P L E M E N T.

THE experiments I had made on the spirit of the cherry-laurel, which, notwithstanding I had found it innocent when applied to the eyes of quadrupeds, such as guineapigs, rabbits, &c. (in the quantity however in which I had tried it) was capable of killing pigeons in a few minutes, when applied to their eyes: these experiments, I say, made me suspect that the oil itself of the cherry-laurel would be a poison to these animals, that are so sensible to the slightest impressions. Here follow the experiments I made on the subject.

I let fall into each of the eyes of a young pigeon three drops of the oil of the cherry-laurel; in the space of a minute it became slightly convulsed; in two minutes the convulsions were become violent and general, and it then fell on its breast without the power of supporting itself. In two minutes more it died. Neither of its eyes nor eyelids were sensibly enflamed; there were circular red vessels however in the uvea, at some distance from the pupil

pupil. Two other pigeons treated like this one, died, one in five minutes, the other in less than seven. It is certain then that the oil of the cherry-laurel, as well as the spirit, is a violent poison when applied to the eyes of pigeons.

These experiments induced me to think that the eyes of pigeons were formed in such a way, or were delicate and sensible to such a degree, that the venom of the viper, which I had found innocent when applied to the eyes of other animals, would make violent impressions on them. Although neither of the pigeons died to which I applied this poison, my suspicion was not altogether without ground; for having repeatedly covered the eyes of two of them with the venom, I soon found their eyelids to swell considerably, insomuch that the eyes themselves were scarcely perceptible, and seemed as if sunk in a hollow. In seven minutes their eyes were no longer visible, and the pigeons were several hours before they could open their eyelids. The uvea and transparent cornea were not inflamed, but the inner part of the eyelids was very much so. The venom of the viper then is not altogether innocent, when simply applied to the eyes of certain animals; although in some others it is not hurtful when applied in the same quantity. I am however persuaded that if it were applied for a long time to the eyes of these last, its effects would be perceived; and that it might even occasion death, or some violent derangements at least.

These

These experiments on the eyes of pigeons, which become inflamed when the venom of the viper is applied in abundance to them; and those I had made on the *ticunas*, which does not occasion death when swallowed, unless it be in a large quantity; confirmed me still more in my sentiment (*a*), that the viper's venom itself might kill animals when swallowed plentifully. Chance having thrown a good number of very large vipers in my way, and those in full vigour, I was determined not to lose the opportunity of establishing for posterity, so important a point of natural history. As it is not the place here to enter into a detail on this subject, I shall for the present content myself with relating, in a few words, an experiment I made on a pigeon, young it is true, but very lively and in sound health.

I cut off the heads of eight vipers, and received the venom expressed from them, in a tea-spoon; it was almost full, and might contain thirty drops or more. I introduced the whole of this into the œsophagus of a pigeon that had fasted for eight hours. In less than a minute it became very weak, two minutes after it began to reel, and at length fell on its side in violent convulsions, dying in less than six minutes. The beak, œsophagus, and crop, to the very gullet, were inflamed and livid, and the blood was blacker than usual; these parts were so discoloured, that they seemed to tend to gangrene and sphacelus.

(*a*) Page 105 of this volume.

It can no longer then be doubted but that the venom of the viper, contrary to what Redi and many other famous observers after him have written, is a violent poison. This venom, like the *ticunas* and several other poisons, when taken in a small quantity, either does not produce, or does not seem to produce, any effect; although it is notwithstanding true that it kills very suddenly, and that in very small quantities, when introduced into animals by the medium of wounds, and of the blood. It is certain that the famous Jacques, the viper-catcher Francois Redi speaks of (*a*) venturously offered to swallow whole spoonfuls of it; but we do not read in any part of the works of that celebrated writer, that the good Jacques had afterwards fulfilled his promise, and he was certainly fortunate in not having done so. The greatest proof Redi gives of the courage or temerity of this man, is his having swallowed the venom of three vipers in half a glass of wine; that is to say, that he swallowed a few drops of it, perhaps three or four only, considering the imperfect method he must have employed to extract it. I am persuaded that the venom of even a more considerable number of vipers, mixed with so large a proportion of wine, would not be dangerous to the person who should swallow it; but on the other hand I am of opinion, that a whole spoonful of this poison, unmixed with other substances, would be very capable of killing a man himself. The expe-

(*a*) Observations on Vipers, published in Italian, at Florence, 1664, Page 17.

riment Redi himself made, proves still less than the other. He squeezed the venom of four vipers into a glass of water, and gave it to a kid without any ill consequences. The water was in a much larger quantity than the wine I have mentioned, and consequently the venom ought to have been far less active, since it was more diluted and more divided. But, as Redi pretends, it does not follow from all this that the venom of the viper, introduced in abundance into the stomach, would be neither mortal nor hurtful. It is both hurtful and mortal when taken in a large quantity. This error is indeed common to the ancient philosophers, who believed that the venom of snakes was only a poison when introduced into wounds: *Non gustu, sed in vulnere, nocent*, says Celsus; and Lucan before him, puts into the mouth of Cato, *Morsu virus habent, & fatum dente minantur; pocula morte carent*.

The venom of the viper, as is said in the first volume, although separated from the animal, retains its hurtful qualities for several months; but a well determined experiment has latterly induced me to believe, that its faculty of killing, if indeed it last so long, does not continue beyond the ninth month. This is the experiment.

I wetted four pieces of thick brown paper, each of them with about twenty drops of venom, and enclosed them separately in glasses. At the end of nine months, I forced these papers into the wounds of the legs of four young pigeons. Neither of the animals died, nor gave any symptoms of the

disease of the venom. The venom thus preserved then had lost the faculty of killing even pigeons, that are so readily destroyed by it when fresh.

On the Lunar Caustick.

Although, as has been seen in the first volume, I had satisfied myself that the fluid volatile alkali is not a specifick against the bite of the viper, and that it does not deprive the venom of this animal of its noxious quality when mixed with it, I had however the curiosity to examine likewise the lunar caustick, and began my researches by mixing the venom of the viper with this caustick substance, to see whether it would still preserve its original venomous quality, as it preserves it when it is united with the fluid volatile alkali. I made my experiments by mixing equal quantities of the venom and lunar caustick, adding a few drops of water to render the paste, which I applied to the wounds of animals, somewhat more liquid. And as small birds are easiest killed by the venom, I wished to try on such the effects of this paste, and procured sparrows and pigeons for that purpose.

Experiments on Birds.

I wounded the legs of five birds with the venomous teeth of vipers, making scarifications immediately,

mediately, and applying the above paste. Neither of them died, nor seemed affected with the disease of the venom; and there was no gangrene in either of their legs, although the muscles were much burnt by the caustick.

I repeated this experiment on five other birds, and in addition, washed the parts with water, after having applied the paste. Here again neither of them died, and I found that the muscles of the legs were less corroded and burnt by the caustick.

As these animals are so small that an atom of venom suffices to kill them, it appears that it can be no longer doubted for a moment, but that the lunar caustick renders the venom of the viper innocent when it is scarcely mixed with it; for I applied the paste to the wounded muscles, the moment I had blended them together. I wished however to try it on ten other birds; and to my great surprise, they all recovered very readily. Still I could not determine as to the unexpected novelty of these consequences; and fearing that accidental circumstances might have prevented the action of the venom, I resolved to make fresh experiments on the same animals. I wounded the legs of six others, multiplying the incisions to introduce a good deal of the venom, of which I am sure the quantity of paste I employed in each must have contained a drop at least. In these experiments two of the birds actually died, one in the space of six hours, the other in twenty-eight. On the morrow I repeated this experiment, with the same circumstances,

cumstances, on ten other birds ; one only of them died, and that at the end of twelve hours. Fearing that the wounds alone might have brought on death, particularly as they were irritated by the caustick, I tried ten birds, on the legs of which I made wounds as usual, and applied the caustick by itself. One of them died at the end of eight hours. So that it seems at least very probable, if not very certain, that the three birds before mentioned, died likewise of their wounds, and not of the effects of the venom. I wounded ten other birds in several places, in the pectoral muscles, to which in a short space I applied the paste. Not one of them died.

Experiments on young Pigeons.

The pigeon, next to small birds, particularly if very young, is the animal killed with the smallest quantity of venom. I chose four of these for a trial, and operated on all of them in the same way. I made several transverse wounds with scissars, in the muscles of their legs, and introduced the venomous paste abundantly into the wounds, which although pretty deep, did not bleed considerably. Neither of these pigeons either died, or seemed to have the disease caused by the venom of the viper. The next day I repeated this experiment on twelve pigeons, the legs of which I wounded in several places,

places, and again applied the paste; neither of them died. I varied the application of the venomous paste, which I sometimes forced into the wounds with small bits of wood, sometimes with pieces of stout thread, smeared with it. Neither of them died in these trials. I proceeded to the muscles of the breast, which I wounded in different ways, and diversified the application of the paste: but it was in vain that I multiplied my experiments; neither of the pigeons died.

It cannot now be doubted but that the lunar caustick, when mixed with the venom of the viper, renders it innocent; and thus every thing concurs to make us regard it as the true and only specifick against this poison. I can now flatter myself with having at length discovered a certain remedy against the bite of the viper; a remedy that so many people have hitherto sought in vain. But does the viper's venom lose its deadly qualities, when mixed with the lunar caustick, because its nature is changed, or rather, because being united with this strong caustick, in the way with acids when they are saturated by alkalies, or by absorbent earths, it can no longer exercise its original qualities? May we not suspect that the lunar caustick, by crisping the blood-vessels, prevents the poison from insinuating itself in this way into the blood? This last suspicion does not seem supportable, since the mineral acids, which likewise crisp the vessels, are however not capable of rendering the venom innocent, and since the fluid volatile alkali itself has

no such property : this must appear strange to us when we consider the great agreement there is betwixt the fluid alkali, and the lunar caustick.

I must confess that I felt a real satisfaction, when I flattered myself that my labours had been crown'd with such great success ; and the knowing that the venom of the viper does not lose its hurtful qualities when united to other very active substances, such as the mineral acids, concurred to inspire me with confidence. But I well recollected the error I had fallen into in France, when I thought I had found a certain remedy against the bite of the viper, because I was able to recover small birds and pigeons that had been bit. Simple analogical proofs had no longer any weight with me, and in the present case only served to make me recur to immediate and irresistible experiment, which alone ought to be consulted in philosophical researches. This is the only purpose to which a prudent philosopher, if he does not wish to be deceived or led into error, ought to apply these proofs ; and 'tis to this application of them that the penetrating naturalist owes his most important discoveries.

Experiments on Birds.

I wounded the muscles of the legs of four small birds with venomous teeth, and after having made slight scarifications, I applied the lunar caustick,
washing

washing the wounds soon after. Neither of them died, nor had the disease of the venom.

I wounded four other birds like the preceding ones, in the legs, with venomous teeth, and afterwards washed and scarified the wounds, but did not apply any remedy. They all died, one in a single minute, one in four minutes, one in seven, and the fourth in eight.

I wounded the muscles of the legs of four others with scissars, and applied the venom to them. Immediately after, I scarified the wounds, applied to them the remedy, and washed them. They all four recovered.

I treated four other birds in the same way with the last, and neither of them died.

I thought it proper to repeat the same experiment on ten others. I wounded their legs, applied the venom, scarified, and medicamented, and all ten of them recovered.

I must not however conceal that of five other birds I had wounded in the legs with venomous teeth, three died, although they had been scarified and dressed with the lunar caustick in the way described above. Two of them died at the end of three hours, the third on the twentieth.

Two other birds died in the same way, out of four, of which the pectoral muscles had been wounded with venomous teeth, and had been dressed in the usual way, after the scarifications had been made. One died in three minutes, the other in three hours.

Another

Another time I wounded the pectoral muscles of three birds with a lancet, and applied the venom. I dressed the wounds with the lunar caustick, and the three birds all died, one in half an hour, a second in eight hours, and a third in nine.

I feared that the wounds made in the pectoral muscles, joined with the caustick, were alone sufficient to occasion death; and therefore wounded these muscles in three other birds, and applied the lunar caustick to them: neither of the birds died.

We may I think conclude from the experiments related above, that the lunar caustick, when applied in the way I have described, destroys the effects of the viper's venom, in birds. If it however does not prevent some of them dying of this venom, it may be believed, either that the remedy has been applied too late, or, which is more probable, that it cannot always reach the envenomed parts by introducing itself as far as where the venom has penetrated. Every one must perceive that in these cases it cannot have corrected the deadly qualities of the venom, as it certainly does correct them, in the way we have seen, when they are united together.

Be that as it may, the lunar caustick being capable of recovering birds, is not a sufficient assurance to us that it would recover other creatures in the same way: and even though this caustick should be the true antidote against the venom of the viper, as it incontestibly appears to be, it does not follow
of

of necessity, that it ought to cure the larger kind of animals. Circumstances may vary, the scarifications may be more dangerous, and the application of the specifick to the envenomed parts, either more difficult, or less certain.

Experiments on Pigeons.

I wounded the muscles of the legs of four pigeons with venomous teeth, made the usual scarifications, applied the lunar caustick, and covered the wounds with linen. Two of the pigeons died in a few hours, the other two survived. I washed the wound of one of the two first, after I had dressed it, but did not wash that of the other; and I observed the same treatment to the two that survived.

I repeated these experiments on four other pigeons, to which I applied the venom taken from the animal, and did not wash either of their wounds. Neither of them died, nor had any appearance of the disease of the venom.

I made the same experiment on six others, and applied the venom to the muscles, after having wounded them. They all survived.

I returned to my first experiments, fearing that the remedy had not reached all the parts where the venom had penetrated, in spite of my having made large and deep scarifications. I operated on the legs
of

of seven pigeons, three of which died in less than an hour, the other four had no complaint.

I passed on to the pectoral muscles. I wounded those of four pigeons in several places, and applied the lunar caustick. They all four recovered.

On repeating the same experiment on four other pigeons, neither of them died, nor became ill. I got ready twelve other pigeons as above, and applied, first the venom, and immediately after the lunar caustick, to the wounds in their pectoral muscles. They all twelve recovered.

I wounded the pectoral muscles of four others with venomous teeth, and immediately scarified the wounds, and applied the caustick. Two of them died in less than an hour.

It cannot be doubted after this, but that the deaths of the animals to which the lunar caustick was applied, were occasioned by its not being invariably able to reach all the parts envenomed; and not because it is no true specificck against this poison.

I must again confess ingenuously, that having one day applied the venom to wounds made in the muscles of the legs of two pigeons, one of them died at the end of four hours, notwithstanding I had treated it with the caustick. Another time I wounded the muscles of the legs of two pigeons, with venomous teeth, and one of them died in eighteen hours. But unless I am deceived, all these cases only tend to prove still more, either that the remedy does not always come in time,
or

or does not always unite with itself the venom : this seems to be sufficiently demonstrated by the time that these animals continue to live, whilst they in general die in a very short space, when the remedy is not applied.

I was desirous of making a trial on ten other pigeons, and having wounded their legs with venomous teeth, I scarified them, and applied the caustick a little after. Five recovered without having had any complaint, a sixth died in my hands, and the other four died, one in three, one in sixteen, one in eighteen, and one in nineteen hours.

I think it quite superfluous to give a detail of several experiments I made on the muscles of the legs and breasts of fowls. I wounded them in several places, applied the venom abundantly, and treated them in the usual way with the caustick. Neither of them died. This may readily be accounted for, since pigeons, which are much more tender, and which sink more readily under the effects of the poison, have in the same circumstances escaped death.

Experiments on Quadrupeds.

I wounded two guineapigs, each of them repeatedly in the muscles of one of its legs, with venomous teeth, and after having made the scarifications, I dressed the wounds with the lunar caustick : one recovered, the other died at the end of five hours.

I treated

I treated four other guineapigs in the same way, and only one of them died, and that at the end of five hours.

I wished to try whether on applying the venom drawn from the animal, to the wounded museles, the remedy would be more certain. I wounded the muscles of the legs of six very small rabbits in several places, and applied the remedy; neither of them died. I treated the pectoral museles of six other rabbits in the same way, and all of them recovered.

I proceeded to small guineapigs, on six of which I tried this remedy. To three of them I applied the venom to the museles of the legs, to the other three to those of the breast, each of which I had previously wounded: I then applied the caustick. Neither of the guineapigs died.

I returned to the venomous teeth, with which I wounded eight very small rabbits in the legs. A little after I scarified and medieamented the parts. Two of the rabbits died; the other six recovered.

It can no longer be doubted but that the lunar caustick is a true specifick against the venom of the viper. But the mode of applying it to the envenomed parts is not certain, and it is natural to conceive that the difficulty will become much greater, when the application of it is attempted on the immediate bite of the viper; particularly if the creature may have bit several times, and if the traces of the bites can scarcely be distinguished. In these
cases

cases there will be always some uncertainty, and if the remedy fail in correcting the venom, too extensive and multiplied scarifications may be extremely hurtful.

Bites of the Viper treated with the Lunar Caustick.

The latter part of my experiments is the more important, in having for its object the securing us against the bite of the viper. My experiments are too few in number, and too little varied, either to allow the drawing from them all the practical utility that may be hoped, or to render the method I have proposed perfect. Owing to the season, I have experienced a scarcity of vipers; and the circumstances in which I have found myself, and the obligations I have had to fulfill, have prevented my applying myself more attentively, and in the way I should have wished, to this subject. I shall for the present publish the result of such experiments as I have been able to make, intending to return at a more convenient opportunity, to an enquiry that has the good of my fellow creatures for its object. In the mean time, I hope that philosophers and naturalists will pay every attention to this branch of medicine, and will spare no pains to render it more certain and useful.

I had a middle sized rabbit bit five times successively in the leg, by a large viper, and, after making scarifications, applied the caustick, and washed and bound

bound the wounds. The rabbit died at the end of twelve hours.

I had another rabbit bit several times in the leg by a viper. It died in the space of an hour, although it was treated like the preceding one.

I had two guineapigs bit in the legs by a viper, each one three times, and after making the scarifications, I applied the caustick. Both of them died in a few minutes.

I repeated this experiment with the same circumstances, on a large guineapig, which died in the space of twenty-four hours.

These five unexpected deaths convinced me how easy it is to be deceived, even in matters of observation and experiment, and how little trust is to be reposed in analogy. The minutest circumstance suffices to render what in itself would be very useful, both useless and hurtful. Every one may perceive, that in the present case the whole difficulty lies in making the caustick penetrate into all the parts to which the venom has found its way. But how can this difficulty ever be surmounted? The holes made by the teeth of a viper are very small, and often invisible. They run in different directions within the skin, and have different depths, according to a thousand varied circumstances. The swelling or inflammation that succeeds, augments the difficulty still more, so that the scarifications are made almost at hazard.

I must not however omit that I have cured five other larger rabbits by this method, after they had been repeat-

repeatedly bit by vipers ; as also several guineapigs that I had had bit in the same way ; all of which would probably have died, if they had not been treated with this new remedy. But I recovered a much greater number of these animals, when they had been bit a single time only, although even in this instance some of them died ; and this was undoubtedly occasioned in the way related above ; that is to say, not by the inefficacy of the medicament, but because it cannot always reach the parts where the poison has penetrated and lodged. There are likewise other circumstances which elude the new method I have proposed, and these are when the disease, by accidental circumstances, is more internal than external ; that is to say, when the venom is suddenly introduced in a large quantity into the blood of the animal, by the means of some vessel that the teeth may have penetrated. And I do not think it impossible but that the bite of the viper may kill even instantaneously, provided it should ever happen (which is not absolutely impossible) that the teeth should pierce a large venous vessel in such a way, that a quantity of the venom would be instantly carried to the heart. In this case, which differs little or not at all from the artificial injection of the venom, the disease may be incurable, and obviate all remedy.

The lunar caustick, I repeat it, renders the venom of the viper innocent, and is its true specific remedy ; but much remains to be done, to apply it with the greatest advantage in the bite of this ani-

mal. It would perhaps be useful to swallow it diluted with water, even in pretty strong doses. If the venom of the viper derange the blood, and be fatal when it is introduced into the torrent of the circulation of humours, the lunar caustick, taken internally in a liquid form, may weaken its noxious qualities, and correct it in the vessels themselves, to such a degree as to destroy, or diminish, the internal disease that this venom produces.

After my having discovered that the lunar caustick renders the venom of the viper innocent, it is natural to conceive that I ought to make some trials on the *lapis infernalis*; I have indeed made several.

I found that a paste formed of this stone, and of the venom of the viper, might be applied with impunity to the wounded muscles of birds; on choosing ten of them for these experiments, not one died. But of three which I envenomed with the teeth, and dressed with the *lapis infernalis* scraped to a powder, two died, one at the moment of the application, the other at the end of two hours. I had four pigeons bit in the legs by vipers, and treated them with the same caustick. One died in my hands immediately after I had applied it, another in the space of an hour, and the two others recovered.

Notwithstanding that the season was becoming unfavourable, and that I had no longer a hope of finding any vipers, I met with thirty-four of them by accident, in an excellent state and very vigorous. The first purpose to which I applied them was that of

verifying my new remedy, and of seeing at the same time whether a solution in water of the lunar caustick given internally, would be at all efficacious to animals bitten by the viper.

I destined four very small guineapigs for this experiment, and made them drink a teaspoonful of the above solution : it was rather weak, but still disagreeable to the taste. I wounded the femoral muscles of three of them with venomous teeth, made immediate scarifications, and applied the lunar caustick as usual ; neither of them died.

I made another small guineapig swallow two teaspoonfuls of the above solution, and it died in my hands. I conclude from this that the quantity I employed was too great. I afterwards gave a single teaspoonful, as in the first experiment, to other four small guineapigs, and had them immediately bit by as many vipers, making scarifications instantly after. They all four died. One when scarce bitten, another in an hour, a third in three hours, and the last in five. The result of this experiment shows that the bite of the viper is far more dangerous, than wounds that may be made artificially with its teeth, although filled with venom. One reason perhaps is, the difficulty of conveying the remedy nicely to all the parts where the teeth of the viper penetrate when it bites at its will. I likewise imagined at the time that the smallness of the animals I made choice of might partly have caused this, and determined in consequence to make trials on larger and stronger ones, better able to resist the effects of the

poison; particularly the internal malady, which is communicated much quicker in small animals. I had six fowls bit in the thigh by as many vipers. Five of them swallowed three teaspoonfuls each of the solution of the caustick, the other did not swallow any. I applied the lunar caustick in the same way to each of their wounds; the last died, the other five all recovered.

I had six rabbits of a middling size bit in the thigh by as many vipers. I immediately applied the lunar caustick to their wounds, and made them all drink the solution of it. Four of them recovered; the other two died, one in three hours, the other in eight.

I repeated this experiment on six other rabbits, somewhat larger than the above, and neither of them died. On having four others bit, and treated exactly in the same way, they all likewise recovered.

The number of these experiments is still too circumscribed to render us certain that the lunar caustick is a never failing remedy against the bite of the viper; and this is owing to the difficulty of conveying it to all the parts into which the venom has insinuated itself: three or four hundred experiments would scarcely suffice to fully clear up this important matter; I however have no doubt of the efficacy of this remedy, and can affirm that the lunar caustick is the true specific remedy against this dreadful poison.

On the Poison called Ticunas.

The singular and unexpected effects of the lunar caustick, which, when united with the venom of the viper, renders it innocent, made me suspect that the effect might be the same if it were united with the *ticunas*. I mixed them then in equal quantities, and formed a somewhat soft paste. I now made several wounds in the muscles of a pigeon's leg, and applied this paste to them. The pigeon died in less than two minutes. I repeated this experiment with the same circumstances on another pigeon, which died in less than three minutes. I again repeated it on two others, one of which died in less than two minutes, and the other soon after the end of the third. The lunar caustick then does not correct the deadly quality of the *ticunas*, and consequently is not its specific remedy. The paste formed of it does not even retard the death of animals to which it is applied; since two pigeons, to the legs of which I had simply applied the *ticunas*, died in three minutes, and not sooner. I pass over in silence many other analogous consequences which I obtained on making like experiments on guineapigs and small rabbits.

Having procured several snakes, similar to those the examination of which is mentioned in page 139 of this volume, for the occasion, I had the curio-

fity to see whether the poisoning them with the *ticunas*, in the muscles of the tail, would be attended with the consequences I had before observed; and whether these creatures would continue in an apparently lifeless state, for as many hours as they did on the former trials. I introduced then into the muscles of the tail of one of these snakes, an American arrow which I had previously steeped in the *ticunas*, liquified by the heat of boiling water. I made a considerable wound along the vertebræ, that the poison might penetrate well into the muscles. At the end of an hour the snake scarcely stirred, and in an hour more seemed dead, and quite deprived of motion and irritability. In this apparent state of death, I examined the motion of the heart attentively through the skin, and could perceive a feeble and slow contraction of this muscle. This contraction continued for twenty-seven hours, constantly diminishing in force, and in this interval every one would have looked on the creature as dead, for setting aside the motion of the heart, it was quite still and free from irritability. At the end of twenty-seven hours the motion of the heart gradually became quicker and more considerable; and I then thought I perceived, on giving the creature a violent blow, a small undulating motion. In forty hours, I observed a motion, sometimes of one extremity of the body, sometimes of the other, but very feeble. In ten hours more this motion, and the return as it were to life, were manifest and certain, notwithstanding the snake could neither

ther crawl nor hold up its head. I left it in this state during the night, and the next morning found it very lively, in good health, and able to crawl; but six hours after, it died.

The event was exactly the same on treating two other snakes in the same way. Another much smaller one died in less than two hours; its heart continuing to move three hours after its losing all apparent signs of life.

It cannot then be doubted but that the *ticunas* is a mortal poison, even to animals with cold blood, although it be true that it is far less so to them, than to those that have the blood warm. But that which well merits our attention, is the apparent suspension of life, and of the voluntary motions of all the animal's muscles, except that of the heart, the irritability of which is indeed diminished, but not totally destroyed.

We cannot afterwards but admire, that a long continuance of this isolated motion of the heart, is capable of restoring by degrees to the animal, that life, and that motion of all its other organs, which it had entirely lost. Without the action of this muscle, all must irrevocably have ceased.

*On the dangerous Consequences of the Oil of the
Cherry Laurel.*

The following experiments on the oil of the cherry-laurel, will not only serve to complete those

I have already made on this subject, but to show in a clearer way, that this oil, whether given internally or applied to the wounds of animals, is one of the most terrible and deadly poisons known. This important truth will, I hope, destroy once for all, the abuse that has crept into several places in Italy, of selling the oil of the cherry-laurel publickly in the shops, to all those who ask for it. It is easy to see how dangerous this custom may be to society ; and this danger is encreased again by the method adopted in selling it. It is usually masked under the title of *Essence of bitter Almonds*, and is found under this name in the printed lists of distillers, who sell it with other essences, oils, and liquors, that may be drank with the greatest impunity. Nay, what is more, this dangerous poison enters into the composition of a liquor for the use of the publick, which is made and sold without any ceremony ; and to prevent all suspicion of the true nature of this poisonous mixture, it is vended under the title of *Liquor of bitter Almonds*, or of *Peach-flowers*, and is even put into milk, and into ragouts. It is true that a small quantity of this poison is employed in them, and that these liquors are not drank like wine and water ; but a poison is always a poison ; and we do not besides know but it may be hurtful when used a long time, although in very small quantities ; and whether it may not give a disposition to certain diseases. I have heard some persons say, that when swallowed it must be an excellent cordial,

cordial, which its truly agreeable and aromatick smell may easily induce them to believe.

Tuscany owes to a philosophick sovereign, the knowledge of this pretended *oil of bitter almonds*, and the advantage of being secured from the abuse that may be made of it : so true it is that philosophy is useful even to sovereigns, and that, according to the wish of one of the ancients, they ought all to be philosophers, or at least to know how to philosophize on occasion !

The Oil of the Cherry-Laurel is a Poison to Vipers.

Having had an opportunity of providing a quantity of the oil of the cherry-laurel, I wished to see what effects it would have on vipers, and accordingly made a large one swallow about ten drops. In less than two minutes it could scarcely drag itself along, and at the end of seven seemed quite dead. In two minutes more it was perfectly insensible to the stimulus of a needle. However on observing attentively, its heart was seen to move, and to rise and sink alternately : it continued this motion, which regularly diminished, for three hours. In these animals we may judge extremely well of the total repose of this muscle, without opening the thorax ; an observation that in many cases may be highly important. We may likewise observe this motion of
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the heart externally in other animals with cold blood, even in frogs, but with greater difficulty.

I have found in general that the oil of the cherry-laurel is a very strong poison, even to vipers, the suddenness of its effects on which, is proportioned to the quantity swallowed. I have seen these creatures die in a very few minutes on giving them thirty or forty drops, in which cases they have sickened and become motionless instantly; and I have found it mortal even when given to them in the small quantity of a drop, or at most of two. It is true that in these last instances, the disease discovers itself very late, and that the viper lives many hours after. 'Tis observed that the muscular irritability is in general very soon destroyed, notwithstanding which the heart continues to move for a very long time, even after the animal ceases to give any sign of life, or sensation. The heart, I do not here speak of the intestines, is an exception to the general rule of the other muscles, and this interesting point of animal physics deserves still more the attention of philosophers, as having been hitherto totally neglected.

The Oil of the Cherry-Laurel is a Poison to Snakes.

I made a snake swallow five drops of the oil of the cherry-laurel, which it had scarcely taken, when I observed it to crawl with difficulty; in less than two minutes

minutes it appeared to be almost dead, since it only preserved a small motion of its tail, which ceased a little after. I stimulated it in different parts of its body without any effect. On opening the thorax, I found the heart and auricles motionless; but when I pricked them with a needle, they began to move, and this motion continued several hours. At length I separated the heart from the thorax, and it instantly ceased to move. However on touching it repeatedly with the point of a needle, it each time contracted itself once, and continued to do so for several hours. It never had a spontaneous motion, and never contracted itself more than once on being pricked with the needle.

*The Oil of the Cherry-Laurel is a Poison to Snakes,
when applied to their Muscles.*

I made a wound of about an inch in length, in the muscles of the tail of a young snake, and applied to it about forty drops of the oil of the cherry-laurel. It died in less than ten minutes, without having been convulsed, and without the smallest sign of irritability remaining in its whole body.

I laid bare a great length of the muscles of the tail of an ordinary sized snake, and wounded them in several places. I applied an abundance of the oil of the cherry-laurel to the wounded parts throughout, and a moment after repeated this again.

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In less than a minute the snake moved languidly and with difficulty, and the motion of the different parts was feeble, in proportion as they approached the tail. In an hour after, however, it had in a great measure recovered its former vivacity. I then applied fresh oil to the same wounds, and in less than a minute it could scarcely stir itself, remaining twisted in the form of a zigzag; however, in half an hour it became as lively as usual.

I applied the oil of the cherry-laurel twice, to the muscles of the tail of another snake, which although it had appeared dead, and remained in that state for several hours, both times recovered itself. However, after reviving the second time, and becoming very lively, it died of itself in a few hours.

This oil then, when it is applied to the muscles of snakes, incontestibly produces violent derangements in a very little time, but is not, however, capable of killing them when they are very large; neither does it, in the circumstances at least in which I have observed it, kill the smaller ones very suddenly, notwithstanding it is otherwise certain, that snakes without distinction die readily, when made to swallow it, even in a very small dose.

It is a Poison to Vipers, when simply applied to their Muscles.

I wished to see whether the oil of the cherry-laurel would be mortal, when applied to the wounds
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of vipers purposely made, and whether it would be less so in these cases than in those I have related, of their swallowing it. It has been found by a long succession of experiments I have made for that purpose, that both the spirit and oil of this plant produce violent derangements when applied to the muscles of vipers, but not to such a degree as when they are taken internally. In these cases the heart continues to move in the same way that it does in the other animals with cold blood, whilst all the other parts remain motionless and insensible to the most active stimulants. In like manner I have observed, that on introducing a few drops of the oil into the natural aperture that is found towards the tail of the viper, the creature died in the same way as in the cases related above, that is to say, with a loss of muscular irritability, but with a continuance of the motions of the heart.

I bathed the muscles of a vipers tail, stripped of the skin for a great length and wounded in several places, with the oil of the cherry-laurel; an instant after, it had lost the motion of the part of the body towards the tail; it twisted and wrapped itself up, swelled considerably, and was violently convulsed.—I likewise bathed the muscles of another viper towards its tail, with this oil, and in twenty seconds it could scarcely move itself. It was contracted and wound up, was swelled to twice its size, and numbed. It died in less than three hours.

It acts likewise as a Poison, when applied to the Muscles of Pigeons.

It will be sufficient to relate here some of the experiments I made on the muscles of pigeons, to show in what way the oil of the cherry-laurel acts in these cases.

I stripped the whole of the skin from the leg of a young pigeon, and wounded the muscles in several places, without dividing any perceptible blood-vessel. I then applied about twenty drops of the oil to the part, which was more than a square inch in superficies. In six minutes the pigeon was no longer well able to support itself on its feet. In three more it was seized with convulsions, and at length fell on its breast. In six minutes more it had lost its strength, but still breathed; and kept open its eyes. After six other minutes it began to stir a little, and remained quiet for the space of twenty more. It at length recovered, and became as well as ever.

I laid bare the breast of a very young pigeon, wounded the pectoral muscles in several places, and applied to them about twenty drops of oil. Five minutes after, the pigeon became very feeble, and almost incapable of supporting itself; it however survived, and had no other symptoms. The wound was very large, and well covered with oil.

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I repeated this experiment on another pigeon, applying twenty drops of the oil, as above. In the space of three minutes it could no longer support itself, and died in five more.

Two other pigeons, somewhat larger, recovered, although treated precisely in the same way with the last.

I returned to my experiments, on the legs. I laid the leg of a middling sized pigeon entirely bare, and wounded the muscles in several places. I then successively applied to the wounds more than thirty drops of the oil: in thirty minutes it could no longer stand, but soon revived, and recovered perfectly.

This experiment made on two other pigeons, was attended with the same success; neither of them died, but on the other hand soon recovered from the feebleness that seized them: however having repeated it on two much younger ones, they both died in violent convulsions in less than three minutes, notwithstanding I had not laid their legs so bare as usual, and had applied a less quantity of the oil.

From all these experiments, it appears that the oil of the cherry-laurel is in reality a poison to animals, when applied in an immediate way to the muscles, by the means of wounds; but that it is in this way far less deadly, than when swallowed.

It is a Poison when applied to the Eyes of Pigeons.

I omit the mention of several experiments I made on the eyes of pigeons ; it will be sufficient to know that the oil of the cherry-laurel is a violent poison when applied to these organs, and that it causes a speedy death to these animals, in the same way with the spirit itself of the plant.

Applied to the Heart, it renders it motionless.

The faculty possessed by the oil of the cherry-laurel of destroying the irritability of fleshy fibres, induced me to try whether, on being immediately applied to the heart, it would render it motionless, even to external stimuli. I therefore let fall a few drops on the hearts of several frogs, the motions of which soon ceased, and could not be restored by the pricks of a needle. The spirit of the cherry-laurel produces the same effect, but less suddenly and more imperfectly than the oil.

It kills, when applied to the Brain.

I was afterwards desirous of seeing whether this oil would be mortal when applied to the brain of frogs, and a few minutes after the application of it observed that the creatures could scarcely crawl: in less than six minutes they died. The heart however continued to move. The brain being stimulated, no contraction of any part ensued, but on forcing a pin longitudinally into the spinal marrow, the feet sprung up violently. This would lead one to believe that the nervous substance, when put in contact with the oil of the cherry-laurel, loses its power of contracting the muscles; but that this poison notwithstanding, is only capable of removing it in the nerves or nervous productions that it immediately touches.

It deprives the Nerves with which it is put in contact, of the faculty of contracting the Muscles.

To assure myself of this I applied it to the crural nerves of frogs, and in less than two minutes observed that one of these creatures on which I tried it, had lost the faculty of contracting its feet, and that when I stimulated these nerves, at the part where

the oil had touched, with a needle, they no longer put the muscles in action. But every time I stimulated them towards the legs, where the oil had not reached, the feet contracted themselves forcibly. The nerves then are not the organ or instrument by which the oil of the cherry-laurel communicates its deleterious qualities to the other parts of the machine, and are themselves only susceptible of them at the precise part to which the oil of the cherry-laurel has been immediately applied. The spirit of the cherry-laurel, when applied to the nerves, produces effects analogous to those of the oil, but not so violent. It is very probable that this action of both oil and spirit is simply mechanical, and that they alike operate in the way with substances that corrode and crisp.

The Oil of the Cherry-Laurel, administered to Leeches, deadens the Part it immediately touches.

I injected the oil of the cherry-laurel into the bellies of several leeches, which immediately deadened, and were no longer susceptible to external stimuli. The same thing happened when I tried the spirit. I injected some of them for almost half their length, preventing the oil from passing further by a ligature, and saw with surprize that the injected half was deadened, and no longer sensible to any stimulus, whilst the other part was alive, and continued in that state for many hours. This
very

very singular phenomenon is not found either in snakes or vipers, every part of which becomes insensible almost at the same time; and this difference may be principally caused by the diversity of motion in these animals, when compared with the others.

I bathed the part of a leech towards the head, with the oil of the cherry-laurel. In less than three minutes there was no motion in this part, whilst the other half was still living and in full action six hours after.

I bathed the part of another leech towards the tail, and in less than two minutes found it motionless; the other part continued to move for six hours.

I rubbed a part of a snake's tail that I had cut, with the oil of the cherry-laurel; in less than half an hour it had lost all motion.

*The Oil of the Cherry-Laurel is destructive to Animals,
when injected by the Jugular.*

The multiplied examples I have related above, of the deleterious powers of the oil of the cherry-laurel, induced me to think (notwithstanding I had two years before in London, injected the spirit of this plant into the jugular veins of rabbits without its causing their death) that it might kill animals, when injected into the blood. I presumed that the

oil being much more pungent and burning than the spirit, would act on the blood with far greater energy. In consequence of this I made several experiments.

I injected into the jugular vein of a large rabbit, ten drops of the oil of the cherry-laurel, with which I had mixed five or six drops of water. At the moment the liquor passed through the syringe into the vein, the animal fell into convulsions and died. I opened the thorax, and found the blood blacker than it is in its natural state. The left ventricle and left auricle of the heart were almost empty, and the little blood that remained in them coagulated. The right ventricle and auricle were swelled, and filled with coagulated blood. There was no motion, neither could any be excited by stimulations. The lungs were covered throughout with large dark spots, and all their vessels filled with coagulated blood, some of which was likewise extravasated in several places. From the part of the liquor that remained in the small syphon, I judge that scarcely seven drops had entered the vein.

I tried the oil on another rabbit in a less quantity, only injecting five drops with as many of water. The rabbit became convulsed and died instantly. I likewise opened the thorax of this one, and found the heart and auricles in motion. The right ventricle and auricle were swelled, and the opposite cavities contained a little blood. In a short space the heart ceased to move, and I found the blood in the right auricle and ventricle somewhat viscous and

and black. In the opposite cavities lay a little blood of a red colour. The lungs were stained with blood all over, but not so much so as in the former case, and the blood in the vessels seemed to be stagnant.

I do not think that more than three drops of the oil entered the jugular vein, and yet we see that the animal died instantly. It cannot here be doubted but that the cause of the death lay in the lungs, and in the blood that was found stagnant in their vessels. It is superfluous to remark that the suddenness of this event, and the certain marks of a general coagulation of blood in the lungs, directly exclude the pretended action on the nerves, and furnish a certain proof to the contrary.

I have since found that if the oil be injected in a much smaller quantity, either death does not ensue, or happens much later ; in this case there are very violent convulsions, which are certainly produced by the anxiety in the animal, caused by the blood's becoming by degrees stagnant in the vessels.

The Spirit likewise of the Cherry-Laurel, kills when it is injected into the Vessels.

On making these latter experiments it was natural to suspect that the spirit of the cherry-laurel, when injected into the vessels, would likewise bring on death, and that the experiments I made in London were not conclusive, because they were too few

in number, or perhaps likewise, because I employed a weak spirit. Be that as it may, I wished to assure myself afresh by experiment, and I do not blush to oppose my own trials to new ones, more decisive, more precise, and more numerous.

I prepared then a spirit of cherry-laurel three times distilled, and having put about fifty drops into the syphon, I injected them into the jugular vein of a rabbit. A short time after the injection, perhaps in less than forty seconds, the creature died in convulsions, which were however not violent. Having opened the thorax, I found the lungs quite spotted; the spots were very small, and resembled so many dots of a darkish red colour. The blood in the pulmonary vessels appeared quite viscous and stagnant, and I found it in the same state in the heart.

This experiment, repeated on three other rabbits, varied but little in the result; one of them died at the moment of the injection,

The spirit I employed was highly active, and killed the animals very suddenly to which I gave it in an exceedingly small dose. I shall not enter into a detail of any further experiments, because those I have just described are sufficient for my purpose; however I met with a case of a middling sized guineapig that particularly deserves to be noticed. I made it swallow a teaspoonful of the spirit of the cherry-laurel of the third distillation, which had scarcely reached its stomach, when it fell as if dead, and remained in this state for six minutes; it then suddenly

denly sprang up and began to run, although with some difficulty. In a few minutes it appeared as strong and lively as it was before it had swallowed the liquor ; I however found it dead two hours after.

It is then beyond a doubt that the spirit of the cherry-laurel itself, given in sufficient doses, and rendered more active by re-distillations, is a violent poison when introduced into the blood by the jugular vein, and that it kills instantly ; so that it is no longer an exception to the law I have established of the other poisons, which when introduced immediately into the blood, without touching either the nerves or the wounded solids, kill in an immediate way, and in a few instants after bringing on convulsions. It is not only absurd to have recourse to the nerves to explain the action of the poison in these cases, but this imaginary hypothesis is quite superfluous, since its violent effects on the blood are so very evident.

The Lunar Caustick does not render the Oil of the Cherry-Laurel innocent.

I wished to know whether the lunar caustick, blended with the oil of the cherry-laurel into the form of a paste, would not serve to correct it.

I made several small wounds in the pectoral muscles of a pigeon, and applied the paste to them. In less than a minute it was seized with convulsions, and died an instant after.

I repeated this experiment on another pigeon, which in six minutes fell into violent convulsions, and died in a very short space.

I made a comparative experiment, to see what effect the lunar caustick alone would have, applied to the pectoral muscles of a pigeon. It was a little disordered by it, but soon recovered itself perfectly, without having been at all convulsed.

I applied the paste I have spoken of, to four other pigeons prepared in the usual way. They all died convulsed in less than five minutes.

It is evident then that the caustick alkali does not correct the deleterious qualities of the oil of the cherry-laurel ; neither does it correct those of the spirit, as I have found by several experiments, the relation of which I here omit.

On Opium.

I was a long time desirous of rendering myself acquainted by experiment, with the effects of opium applied to the living body. The little uniformity found in the authours who have treated on the properties of this substance, was a strong incentive to my applying myself seriously to so interesting a subject. The experiments I had tried of the immediate application of opium to the nerves, of which mention has been made in the second volume of this work, were too few in number, and too little varied,

varied, to enable me to speak with assurance, and without the risk of having myself been misled by them, on this matter. A little leisure when I least expected it, at length enabled me to make a great number of experiments on opium, of which I shall at present content myself with giving the most general consequences, with a small detail of necessary particulars, to enable my readers to form a clear judgment on the subject.

I had announced several years ago, that opium, dissolved in spirit of wine, and applied to the crural nerves of frogs, deprives them of the faculty of contracting the muscles ; and that this effect ought solely to be attributed, not to the opium, but to the spirit in which it was dissolved ; since experiment had demonstrated that opium, simply dissolved in water, did not in any way derange the nerves to which it was applied. The famous Haller availed himself of the experiments and conclusions I have mentioned, in several parts of his publications against the English physician Robert Whytt, who every where supported the immediate action of opium on the nerves themselves.

The various experiments which other naturalists have made since me, and which are not very conformable to mine, and the different hypotheses which authours have latterly supported on the action of opium, have obliged me to repeat several of the experiments I made a long time ago, and to give them a greater extension and certainty.

I thought

I thought it proper to begin with animals that have warm blood, and to apply the opium to the different organs and parts of the living body. And as spirituous substances are one of the best dissolvents of opium, I wished in the first place to examine all the effects of a solution of it in spirit of wine. I employed an ounce of opium and three of the spirit well united together, and heated *in balneo mariæ*. I likewise made a solution of opium in water without a drop of spirit of wine, employing the same proportion of water that I had done of the spirit, mixing it well with the opium in a mortar, and keeping it several minutes in a jar *in balneo mariæ*; I then added fresh water according to the exigencies of the different experiments.

Result of the Experiments.

Guineapigs which I made swallow a spoonful of spirit of wine, instantly became motionless, and died in less than 20 minutes. Those that swallowed the spirituous solution of opium, became motionless in a few minutes, and died in less than twenty-seven.

Those into the bellies of which the spirituous solution was injected, became motionless in an instant, and all without exception died in less than half an hour.

Those beneath the skin of which I injected the same preparation of opium, died in less than half
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an hour ; and I had scarcely made the injection, when they could no longer stir their hinder feet.

Those into the anus of which I injected it, at the end of half an hour could no longer support themselves, and died in an hour.

Those that drank the spirituous solution died in three hours ; they had scarcely swallowed it when they became insensible and motionless.

Those into the bellies of which the aqueous solution was injected, died in less than two hours ; they lost the greater part of their motion in less than half an hour, and were violently convulsed.

Those, beneath the skin of which the same solution was injected, died in three hours. In the space of half an hour they had lost all motion, particularly that of their hinder feet.

Those that I made swallow this aqueous solution, were very soon incapable of moving ; but only two died, of ten on which I tried the experiment.

Those into the anus of which the aqueous solution was injected, died in less than three hours ; in the space of half an hour they could no longer support themselves on their feet.

It is then a fact, confirmed by all my experiments, that opium, when simply dissolved in water, kills animals with warm blood ; although it is besides true, that when dissolved in spirit of wine, its effects are much quicker and more violent ; but they then proceed, in a great part however, from the spirit itself, since we have seen that this spirit is alone capable of producing all these effects, and that
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it even produces more violent ones, and that more expeditiously.

My experiments on opium, either dissolved in spirit of wine, or simply in water, have been hitherto made on animals with warm blood. I was desirous of repeating them, varying them, and rendering them general, on those that have the blood cold, and presumed that I should obtain consequences equally new and important, since I was about to operate on creatures much more irritable than the others, and in which life is of longer duration, and more tenacious.

I chose turtles and frogs, in preference to several other animals, and likewise made some trials on leeches; a very singular creature, and one which differs, as well in its organs as in its vital functions, from all other known animals.

Result of several Experiments made on Leeches.

Leeches immersed in spirit of wine, died in two or three minutes.

Leeches immersed in a solution of opium in spirit of wine, died nearly in the same space of time.

Leeches immersed in a solution of opium in water, died pretty nearly in the same space of time.

I plunged half the body of a leech into spirit of wine, and found in a little time that this part had lost all motion, whilst the other half continued in action.

action. The experiment succeeded in the same way, whether the part of the leech towards the head was plunged, or that towards the tail.

The same consequences ensued on plunging a leech into the solution of opium in spirit of wine, or into the solution of it in water; and I looked upon it as something very extraordinary, that one half of the creature should become dead, whilst the other half continued in the state of not having undergone any change, or suffered any injury. As to the action of opium on these animals, it appears certain that it should be considered as that of a very violent poison.

Experiments on Turtles, made to swallow different Substances.

A turtle which swallowed spirit of wine, died in less than 20 minutes.

Another turtle which swallowed the solution of opium in spirit of wine, died in the space of an hour.

Another, after having swallowed the aqueous solution, preserved its vivacity for four hours; it died at the end of ten.

I repeated these three experiments with the same circumstances on six other turtles, and the consequences were perfectly analogous to the preceding ones.

It

It is easily seen that opium, although dissolved in water, acts violently on these animals, so as even to kill them; but [that its action is but trifling, when compared with that of spirit of wine.

Injectiōns made into the Anus of Turtles.

I injected three turtles of the same size, at the anus, by means of a small glass syringe. On the first of them I tried spirit of wine, and in a few minutes it could scarcely stir. At the end of an hour, it was quite dead.

I injected the second with an equal quantity of a strong solution of opium in spirit of wine, and in half an hour it scarcely seemed alive or able to stir: it died at the end of the seventh hour, but the motions of the heart continued for an hour after.

I injected the third with the same quantity of the aqueous solution: it was very lively at the end of the sixth hour, and survived the sixteenth.

I have however observed in general, that turtles do not die when injected at the anus with the solution of opium in water. Those I injected with the spirituous solution, all died in less than three hours: the injection was scarcely made when they lost their strength and vivacity, and in half an hour they almost ceased to give any signs of life.

The action of the aqueous solution of opium, when introduced by the anus, is clearly proved; but it is
very

very slow and weak, when compared with that of the spirit of wine.

Injection made beneath the Skin of Turtles.

I made an opening with a lancet in the skin betwixt the legs and belly of a turtle, and injected within it spirit of wine. In a few seconds the turtle became motionless, and died in less than an hour.

I injected another turtle in the like way, with an equal quantity of the spirituous solution of opium. In seven minutes it became without motion, and died at the end of four hours.

I injected a third with the aqueous solution of opium. It continued lively for two hours, but died at the end of the eighth.

The result of a repetition of the same experiments on nine other turtles, was perfectly analogous to that of the foregoing; so that there can be no longer any doubt of the action of an aqueous solution of opium, injected beneath the skin of turtles.

Turtles in which the Heart was laid bare.

I wished to see what changes would be wrought in the heart of a turtle, on applying to it opium, and spirit of wine.

I stripped the pericardium from the heart of a turtle, and applied to it spirit of wine several times successively. In twenty minutes it had lost all motion, notwithstanding the animal continued alive. It died however in less than an hour, when no part of its body was any longer irritable.

I applied the spirituous solution of opium to the heart of another turtle, prepared as above. In half an hour it was become motionless, even to stimulation. The animal died at the end of three hours.

I applied the aqueous solution of opium to the heart of another turtle, and it continued its motion perfectly well for two hours: it still stirred a little at the end of the sixth. The turtle lived eight hours.

I applied an aqueous solution of the cortex to the heart of a fourth turtle; it had still retained a little motion at the end of the sixth hour. The turtle died on the eighth.

I laid bare the heart of another turtle, and sprinkled it successively with several drops of spirit of wine. The two auricles instantly ceased to move, and the heart in less than two minutes had no longer any motion, even when stimulated. The turtle lived a long time in this state.

I opened the thorax of three turtles, on the heart of one of which I poured the aqueous solution of opium, on that of another the spirituous solution, and on that of the third the liquid laudanum of Sydenham. That on which the spirit of wine was poured continued to move several hours longer

than the other two, which ceased their motion as if it had been by concert. The contractions of the auricles of the heart on which I had poured the laudanum, were for some time two by two, they then became three by three, soon after which the animal was quite dead. The heart continued a longer time contracted, in proportion as the intervals betwixt its contractions were of greater duration; a new and singular phenomenon, which cannot be readily explained by the common theories.

These experiments are not sufficient to determine that a solution of opium in water has no action on the heart; particularly as, when it is applied during the time this viscus remains in the thorax, there are vessels and blood that may serve to introduce it into the torrent of the circulation, and carry it to the other viscera, by which a derangement of the animal economy may be brought on; so that we cannot in this case attribute what may perhaps be the effect of a change in the blood, or of some other unknown cause, to its immediate application to the heart: to render the experiment decisive and unequivocal, it must be made in such a way, that all the other parts being excluded, the heart may alone feel the action of the opium. It struck me then to pursue the following method:

Turtles, the Hearts of which were detached from the Thorax.

I removed the heart of a turtle from the thorax, and covered it with spirit of wine. Its motion ceased in a few minutes.

I put the spirituous solution of opium on the heart of another turtle. In the space of a quarter of an hour there was scarcely any contraction of it, and in twenty-six minutes it no longer stirred, even when stimulated.

I plunged the heart of another turtle into the aqueous solution. It still continued to move, but not forcibly, at the end of half an hour. In two hours all was at rest.

I plunged another heart into simple water, and it still retained a little motion at the end of the third hour.

Another plunged into the aqueous solution of the cortex, ceased to move at the end of two hours.

I made three other experiments on the heart, separated from the thorax, and plunged into the spirituous solution of opium, and could not perceive any sensible difference in the diminution and loss of its motion, when compared with that of three others, two of which I had immersed in the solution of the bark, and the third in water.

It seems then very probable, if not very certain, at least from the few experiments I have related, that a solution of opium in water has no immediate action on the hearts of turtles: a new and important truth, contrary to the opinions of the greatest naturalists of this age, and which should induce others that have more leisure than myself, to investigate this important matter, by multiplying their experiments, varying them many different ways, and guarding themselves as much as possible against all accidental circumstances. Care must be taken that the opium may not form a glutinous fluid, and that it may not dry speedily when applied to the part. I avoided this inconvenience by wetting the part from time to time with water.

Frogs that have been made to swallow Opium.

I made a frog swallow about forty drops of spirit of wine. In forty minutes after I found it dead.

I made a second swallow forty drops of the spirituous solution of opium. At the end of forty minutes it was dead.

I made a third swallow the same quantity of the aqueous solution, twenty-five minutes after which it scarcely stirred. It was then lying on its back with its legs stretched out. It died in somewhat less than three quarters of an hour.

On repeating these experiments on twelve other frogs, I found a very sensible difference, but not such as to enable me to say to a certainty, that the aqueous solution of opium itself, kills frogs in a little space, and that it causes convulsions and a drawing back of the muscular parts.

Frogs injected beneath the Skin.

I injected a frog beneath the skin with spirit of wine. It died a minute after.

I injected a second with the spirituous solution of opium, and in a short time it could not support itself on its feet. It however was able to crawl a little at the end of thirty-five minutes, and died on the fortieth.

I injected a third with the aqueous solution of opium. In the space of ten minutes it scarcely stirred, and had its legs stiff and extended. It died in forty minutes.

I repeated the same experiment on several other frogs, and the consequences were pretty much the same. It is certain then that the aqueous solution of opium kills animals, when injected beneath the skin.

The Hearts of Frogs laid bare, but still remaining in the Thorax.

I covered the hearts of three frogs with the aqueous solution of opium, and a fourth with water alone, by way of a comparative experiment. I could perceive no great difference in the cessation of the motion of the four hearts.

In six other frogs I found that the motion had ceased somewhat sooner in the hearts to which I had applied the opium; but having again repeated the experiment on six others, I found on the contrary that the motion of those hearts to which I had applied the opium, ceased much sooner than those to which I had applied the solution of the cortex; so that I cannot conclude from these experiments, that opium, in frogs however, diminishes the irritability and motion of the heart.

To enable me to establish something more certain, I made the following experiments.

Hearts of Frogs separated from the Thorax.

I put the heart of a frog into spirit of wine. Its motion ceased in two seconds.

I put a second heart into the spirituous solution of opium. It ceased to move in twenty seconds.

I put a third into pure water. It continued in motion for forty minutes.

I put the hearts of three frogs into pure water. The motion of one of them ceased in twenty-one minutes; but it spontaneously recovered its oscillations, and that repeatedly.

The second lost its motion in ten minutes, but recovered it of itself.

The third ceased to move at the end of fifty minutes.

I repeated these experiments on upwards of fifty hearts separated from the thorax. I endeavoured to observe the same circumstances in all of them. I put several of them into the solution of the cortex, others into pure water, and others again into the aqueous solution of opium. The consequences have been very diversified, and very inconstant; I could not however conclude that opium has a real action on the hearts of these animals, when applied in the above manner. Such at least is my present opinion, and it is my design to make at a future period a greater number of experiments.

It now remained for me to make a new kind of experiments, which are perhaps the most important, and the object of which is to examine whether opium acts on the nerves.

Frogs, the Brain of which was laid bare.

I laid bare the brain and spinal marrow of a frog; and applied to them spirit of wine. In ten minutes the frog could scarcely move, and was dead in thirty-five.

I applied pure water to the brain of another frog prepared in the way with the preceding one. It was still lively twenty-four hours after.

I applied the aqueous solution of opium to the brain of a third, which at the end of twenty minutes moved with difficulty.

I applied to another the spirituous solution of opium, and thirty minutes after, it was still able to move a little.

I applied the aqueous solution to another, which in forty minutes was drawn together, and stirred but little. It died at the end of the fifty-seventh.

I applied the same solution to another, and thirty minutes after found it contracted, with the body drawn backwards, and the hinder feet stretched out and lengthened.

I applied spirit of wine to another, which died at the end of ten minutes. The heart however continued still in motion.

Another treated in the same way died in twenty-seven minutes, and another again in forty-five; both of them after a few minutes moved with difficulty.

On repeating this experiment on another frog, it died in fifty-four minutes; at the end of the seventh it fell into convulsions, and could neither walk nor support itself.

Another treated in the same way was seized with very strong convulsions, and after four minutes was no longer capable of walking.

The crural Nerves of Frogs laid bare.

I opened the belly of a frog, laid bare the crural nerves, and applied the spirit of wine to those on the right side. At the end of four minutes I repeatedly stimulated the right foot, which remained constantly motionless.

To those of another I applied the spirituous solution of opium. After eight minutes I found that the right foot could no longer support itself, in what way soever I stimulated it; but scarcely did I stimulate the nerves of the left side, when the left foot contracted itself perfectly well.

I applied to the crural nerves of the right side, in a third frog, the aqueous solution of opium. At the end of two hours, the right foot stirred on stimulating these nerves, but not so well as the left foot did, on stimulating the nerves of that side.

I applied the aqueous solution of opium to the crural nerves of the right side, in three frogs, and simply applied water to the corresponding ones of the left; the motions as well of the muscles of the right side, as those of the left, ceased indifferently,

The crural Nerves of Frogs, to which a partial Application was made.

I applied spirit of wine to one side only of the crural nerves. At the end of nine minutes, they could no longer contract the feet when stimulated on that side ; but stimulations on the other side had a contrary effect.

I again applied the spirit of wine to one of the crural nerves only. At the end of four minutes they no longer contracted the feet, which moved however when stimulated at the part where the spirit of wine had not touched. On touching the other nerves, the muscles every where contracted themselves very well ; a proof that the action of this fluid does not extend to beyond the parts that are touched by it.

I got ready a frog for a comparative experiment, to the nerves of which I applied nothing ; at the end of forty minutes it still contracted its feet.

The foot of another frog, four minutes after I had applied spirit of wine, no longer contracted itself, unless when the part of the nerves towards the legs and thighs, where the spirit had not reached, was stimulated.

*The Crural Nerves of Frogs detached from the
Vertebræ.*

I cut the crural nerves at their going out of the vertebræ, and applied spirit of wine to those of the right side. At the end of two minutes the right foot, although stimulated, and its nerves pricked, no longer contracted itself; whilst the nerves of the opposite side were scarcely touched, when the left foot contracted itself forcibly.

I applied the aqueous solution of opium to the right side of another frog prepared in the same way. At the end of thirty minutes I found on irritating the nerves, that the corresponding foot moved, but not so forcibly as the left foot on irritating the nerves of that side.

I plunged the crural nerves of another on one side into the above solution of opium, and those on the other side into pure water, and found each of them, when irritated, to contract the feet alike, even at the end of fifteen minutes.

These experiments are not at all decisive; they may however furnish matter to a variety of reflections. Notwithstanding it appears indubitably that opium, even when simply dissolved in water, whether it is introduced into the stomach by the œsophagus, or into the intestines by the anus; whether it is injected beneath the skin, or into the
abdomen;

abdomen; whether it is applied to the brain, or to the medulla oblongata; acts on the animal body: yet a doubt remains whether its action and energy are wrought on the nerves, or whether it needs the vehicle of the blood and circulation, and the motion of the humours, to give it activity. We have seen that the venom of the viper acts in no other way than by the medium of the blood; and the two vegetable poisons of the *ticunas* and cherry-laurel appear to act in the same manner. It is certain that all poisons, as well as opium, kill when swallowed; but this does not prove that their action is wrought immediately on the nerves, and that they do not employ the medium of the blood. There are many passages open in the mouth, in the œsophagus, in the stomach, and in the intestines, through which the most nimble and active particles of these poisons may introduce themselves readily into the blood. Thus the difficulty which arises from the mortal effects of opium when taken internally, does not prove that it acts immediately on the nerves; and we have besides demonstrated that the three poisons, the venom of the viper, the *ticunas*, and the cherry-laurel, have no immediate action on the nerves themselves.

To be enabled to make some very probable assertion on this difficult matter, an experiment must be imagined in which opium may act freely against the nerves, without the smallest introduction of it into the blood, or rather, without its touching the blood-vessels. Such an experiment, considering the

the dexterity and precision it requires, is not one of the easiest to make, and can be only well tried on very small animals, and on a very few of the nerves. I could think of nothing that would answer my purpose better than the crural nerves of frogs. To obtain certain consequences, and such as do not proceed from deceitful and variable experiments, it was necessary to make a great many trials, to exclude all the preparations that accidental circumstances might have rendered imperfect, to compare the different consequences with each other, and to weigh them in each case, with those of the experiments intended to serve as comparative ones.

The following is the plan I have pursued in making these experiments. As the number of them already exceeds 300, I shall regard the inferences I have drawn from them as certain, till some one shall be able to demonstrate to me the contrary.

I open the belly of a frog, and lay bare the crural nerves, by means of small pincers and scissars, in such a way that they are entirely freed from every other part. I then cut the vertebræ and body of the animal in two, precisely at the place from whence these nerves go out, which, by gently agitating the neighbouring parts, fall betwixt the thighs of the animal. In this state, I remove the thigh bones, and totally clear the nerves for the length of eight or ten lines, and in very large frogs of even more. I let fall the nerves of each thigh into a small hollow glass, which receives them in
such

such a way, that I can fill each glass with a fluid of any kind without its touching the adjacent muscles; the nerves are not only freed from each other, but likewise from all contact with the thighs. I usually put into one of the glasses, such a proportion of whatever I wish to try on the nerves, as to cover the greater part of them with it, without its being possible for any of the liquor to find its way to the thighs, and mix with the blood. I take the precaution not to leave any vessel fastened to the nerves, and to put a little water into the other glass, to keep the contents of it wet like those of the first. In this way I can make a comparison betwixt the nerves that are envenomed, and those that are not, compute the time that they continue to contract the muscles, and judge of the vivacity of the motions.

I destined 300 frogs for these experiments, and divided them into ten classes, according to the different intervals of time that I kept them under trial. Thus I kept the crural nerves of the first class, which as well as all the others was composed of thirty frogs, in contact for ten minutes, with an aqueous solution of opium on one side, and with water on the other. Those of the second remained in the same state for twenty minutes, and so on to an hundred, after which time the nerves were no longer capable of contracting their muscles. It is true, that in some other experiments I have found that 100 minutes were not sufficient for the nerves to entirely lose the power of contracting the muscles; but these
different

different consequences depend upon a thousand particular circumstances, and do not invalidate the law of effects that I have observed in this series of 300 experiments.

This is the result of the trials I have made. At the end of the first ten minutes, I stimulated the medicated nerves; I shall distinguish in this way those to which I applied the opium, and those which were not medicated, and found that the two feet, right as well as left, contracted with the same force and vivacity.

At the end of twenty minutes, I tried the stimulation on the second class of frogs, and could perceive no sensible difference betwixt the motions of the two feet, which were almost as lively as those in the first experiment.

At the end of thirty minutes the motions of the two feet were feebler, but alike in both.

At the end of forty minutes, the feet scarcely contracted; but their distinct muscles were clearly seen to contract, when the crural nerves were stimulated; and the motions of these muscles were equally lively in each foot.

At the end of fifty minutes, the muscles were distinctly seen to move, but not so much as before. The motions were however equal in the muscles of both feet.

At the end of sixty minutes the motions were very small, but alike on both sides.

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At the end of seventy minutes a great deal of attention was necessary to distinguish them to a certainty, but I could find no difference betwixt the motion of the muscles of the right foot, and that of those of the left.

At the end of eighty minutes there was no longer any motion to be observed in several of the frogs, in whatever way I stimulated either their crural nerves that were medicated, or those that were not so. But in the others of this class, I could not find that the medicated nerves were less ready to contract the muscles, than the others.

At the end of ninety minutes, very few of the frogs retained any motion, on their crural nerves being stimulated; and in the thirty that formed this class of the experiments, I could not find that the opium had wrought any greater change on the nerves, than the simple water.

At the end of 100 minutes the muscles of the legs were immoveable, in whatever way they were stimulated on either side.

I can conceive nothing more decisive and more certain than the series of experiments I have just related, from which it seems of necessity to follow, that the circulation of blood and humours in the machine is the vehicle for opium, and that without this circulation it would have no action on the living body.

The aqueous Solution of Opium injected into the Blood-vessels of Rabbits.

It now remains to be seen whether opium, when injected into the vessels, causes death, and whether it produces the same derangements in the animal economy, when introduced into the circulation of the blood, as it does when swallowed, or injected into the different organs and viscera.

I injected about eighteen drops of the aqueous solution of opium into the jugular vein of a large rabbit. It was scarcely injected when the creature could no longer support itself, or walk, and its legs were bent and drawn aside. It recovered in a few hours. I conceive that scarcely eight drops of the solution entered the vein.

I repeated this experiment on a second rabbit, the legs of which, as in the first instance, were bent and drawn aside. At the end of two minutes, it fell on its breast, moving itself a little now and then. After half an hour it ran about freely, and became perfectly well.

The injection tried on another rabbit did not succeed, since the opium, instead of entering the jugular vein, passed wholly into the cellular membrane. This rabbit had no ailment.

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I injected into the jugular vein of another rabbit a tea-spoonful of the same aqueous solution, and it died instantly.

I repeated this experiment on another rabbit, with the same quantity of the solution, that is to say about forty drops; the creature died at the moment of injection.

I repeated this experiment on another rabbit, with the same dose of the solution, a great part of which, however, flowed back during the injection. The animal could no longer walk, nor support itself on its feet, which were stretched out. It died at the end of two hours.

I conceive it to be altogether superfluous to relate, for the present however, a greater number of experiments on opium, injected into the jugular vein, and introduced into the circulation, without its touching any of the wounded solids. When once it is received into the vessels, I do not see how it can communicate itself in an immediate way to any of the nerves, since anatomy assures us, that the inner membrane of the blood-vessels is not furnished with what can properly be called nerves; and although this should even be the case, opium, on touching a nerve, does not act on it in any way, nor produce any derangement in the animal economy, in whatever way it is applied to a nerve, whether that nerve is entire, or cut; whether it is covered with its proper coats or sheaths, or its medullary pulp itself is put in contact with the poison. In

all these cases, without any exception, opium has been found innocent.

Thus then, opium injected into the veins, produces heaviness, convulsions, and, at length, as has been seen, death itself. Wine produces pretty nearly the same effects. Spirit of wine diluted with water, likewise produces heaviness and convulsions, and if genuine and rectified, kills in an instant. The blood is then found congealed in the vena cava, in the auricles, in the right ventricle, and in the lungs; certain effects, and sure causes of death, without our being driven to any recurrence to the nerves.

Emeticks and purgatives when injected, bring on vomiting and stools, as if they had been taken by the mouth; a proof that their action conveys itself unaltered to the stomach and intestines, without the concurrence of the nerves, and in the same way as if these substances had been simply swallowed. And why not say as much of opium, when it is swallowed too? If there is no recurrence to the nerves in the cases of emeticks and catharticks, and there can be no reasonable recurrence to them, why should the nerves be employed to explain the action of opium, whilst this substance, when applied immediately to the naked nerve, does not act on it in any manner, and produces neither change nor derangement in it? I do not however think, that any one will recur to the nerves, in the cases in which opium, injected into the jugular vein, kills, as has been seen, instantly.

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I do not pretend to exclude by my reiterated experiments, any other than the immediate action of opium on the nerves ; and my aim is to prove, at the same time, the immediate action of opium on the blood, independent of the nerves, without perplexing myself with the imaginary hypotheses that neurologists may invent to support old errors and prejudices, and to make them agree with the principles I have just established. Real physicians have now a basis of certain experiments on which they may for the future found their theories on opium, which has been the subject of so much discussion, and which is still so little known ; and I flatter myself that they will resolve to set aside the hypotheses and received opinions they have imbibed in the schools, and reflect maturely on the circumstances I have just related. I know the power of a prejudice in favour of old erroneous doctrines, and how great the resistance is even to the most certain and luminous experiments. The man who is at length convinced of the truth of facts, which are always irresistible, will not listen to the most direct consequences. Prejudice has certainly a great share in this repugnance ; but it is, above all, our self-love that dreads to adopt new truths, because they carry with them a tacit avowal of our ignorance ; hence arises the difficulty of bringing those who are advanced in years, and men of learning who have already acquired a reputation, to admit of new discoveries.

Let not the suddenness of the effects of opium, and the insensible diminution of its weight, be brought in favour of the nerves, against the blood : since it has been seen that the venom of the viper, the Ticunas, and the Cherry-laurel, when injected into the jugular vein, act instantly, so as to occasion death, even when employed in very small quantities ; and since it has been found by experience that the action or effect of these poisons is wrought on the blood, and not on the nerves. Oil of vitriol kills, when injected into the blood, even in a very small quantity, and no one, I apprehend, will say that this liquor acts on the nerves, and not on the blood. Common oil, and many other innocent substances, when injected in the same way into the blood, kill even very suddenly, and produce very strong convulsions. Every one must perceive, that all the derangement these bodies can bring about in the animal economy, is simply mechanical, and dependant on the stoppage or diminution of the circulation in the different viscera, and not on an affection of the nerves. We must not even be astonished at observing very great derangements produced by very small quantities of matter, since the active part of bodies, and particularly of medicaments, is absolutely confined to very small masses, I can almost say, to atoms. And I cannot conceive how any force can act on the nerves, and occasion the greatest disorders in them, and can yet in no way act on the blood, whilst we see that the $\frac{1}{10000}$ part of a grain of the venom of a viper is sufficient to kill a bird,

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when mixed with its blood; and perhaps that which renders this animal gum poisonous, may still again be no more than the $\frac{1}{1000}$ part of this fraction of a grain.

Dr. Robert Whytt starts, as an argument against the blood in favour of the nerves, that when the heart of a frog is removed, the opium it is made to swallow acts equally against sensation and motion; but that when the head is cut off, and the spinal marrow destroyed, the opium operates slower and less violently. The first part of this reasoning is, as has been seen, altogether false, and the other part, even if it were true, would prove nothing; because when the brain and spinal marrow are destroyed, the animal economy may be so changed, that the opium can no longer act, as it previously did, in a state of health. Indeed purgatives, emeticks, and poisons in general, only act on living animals. But in the present case, Whytt's experiment is not conformable to mine, which I have however repeated several times with the utmost attention. Here again, a few experiments can decide nothing, considering the great diversity that attends the event of them.

To render the experiment more simple, and subject to fewer difficulties, I did not cut off the head of the frogs, but made a small opening in the cranium, through which, with a large pin, I destroyed the whole of the brain and spinal marrow. In this way I avoided the great loss of blood that the animal sustains on its head being cut off, and facilitated the comparison with the frogs that had been made

to swallow opium, without the brain and spinal marrow. I noted the duration of the heart's motion, and from time to time stimulated the crural nerves of all of them. I can certify, that having prepared forty-eight frogs, twenty-four in one way, and twenty-four in the other, I could not perceive the opium to act later, or more feebly, in one case than in the other.

I however deduce from these consequences two very important corollaries ; the first of which is, that the motion of the heart neither depends on the nerves, nor on that chain of sensations that constitutes animal life. The second is, that the action of opium is independent of the nervous system.

I find in some authours a strong argument against the blood, in favour of the nerves, in the case where opium is injected into the vessels : it is, that the action of this substance is suddenly carried to the nervous extremities of the blood-vessels themselves, and from thence to all the other parts of the nervous system. It cannot be denied but that fleshy fibres are observed in the large blood-vessels, whence it is certain, that there are likewise nerves in these parts, since there is no muscle without a nerve. But these fleshy fibres are only observed in the large trunks, and not elsewhere ; and it would be absurd to imagine a structure that is belied by observation, with the only view of supporting an hypothesis which is combated on so many sides. It is certain that no nerves are seen to go towards the blood-vessels,

vessels, to unite themselves with them, since the greatest anatomists have not been able to find any. On another hand, the sensibility of these vessels is in no manner demonstrated, and I have succeeded in tying them many different ways, without the animal's giving any symptom of uneasiness. Indeed very great attention must be paid in making these experiments, which are exceedingly nice, that the vessel at the place where it is tied is well cleared of all the neighbouring parts; that it is not tied at a part which may be accidentally crossed by a nerve in its passage elsewhere; and that in tying it, neither the vessel itself, nor the adjacent parts, is dragged. I must likewise recommend the not operating on very large vessels, since I have sometimes observed, that if a large torrent of blood is suddenly stopped, the animal appears to suffer from it. It is, in short, a thing known to all the world, that the internal membrane of the vessels is neither muscular nor nervous, and therefore opium cannot act immediately on the nerves, from the sole cause of its being put in contact with the internal coats of the vessels.

I wished to see whether opium, when swallowed, would diminish the force and velocity of the contractions of the heart, since it appears to have no action on this muscle, that relates to the duration of its motions. I must confess, that I could establish nothing certain on this point, notwithstanding I directed upwards of one hundred experiments to this object alone. I found a great variety and in-

constance in those on the frogs, on which I principally operated. I observed in general that opium given to animals with warm blood, in moderate doses, increases the force and motions of the heart; but that if it is given in a great dose, it seems at once to diminish the vigour of the animal, and the force itself of the heart: in this it resembles many other substances that tend to destroy life, and to abate the vital powers. The action of opium is thus found to be quite conformable to the symptoms that we observe in man, when it is taken internally. The oscillations of the heart, far from being diminished, are most frequently increased; and the few contrary instances that may be found, do not in any way change the general law of the action of opium on animals.

I made twelve frogs swallow each about twenty drops of the aqueous solution of opium, and instantly separated the heart from the thorax. I opened the thorax in twelve others, but did not remove the heart; all of these, as well as the others, had previously swallowed opium. I noted the time of its action on all the twenty-four, and found that the effects of the opium discovered themselves much sooner in the frogs, the heart of which remained in the thorax, than in those from which I had removed it. The difference in time was more than one-half. By the effects of opium, I mean the faculty it possesses of rendering the limbs paralytic; that is to say, of depriving the animal of the power of exercising its muscles. I do not here speak of the heart

3.

heart itself, which continues to move for a very long space of time, even after the animals are dead; nor of the nerves, which, on being stimulated, are yet capable of contracting the muscles, although the creature can in no way move them of itself.

We must distinguish, then, the voluntary motions of the animal, from those that are excited by an external stimulus acting on the nerves, the spinal marrow, and brain. The latter are not always wanted, when the former no longer exist; but in all cases where the latter are not observed, the former are infallibly wanted.

There is another thing to be distinguished in speaking of the nerves, and of motion; this is the sensation, of which the nerves are the only organ in animals. In the course of my experiments I have frequently observed, that when the animal could no longer move any of its parts, if I stimulated its nerves with needles, or applied pincers to them, it gave very certain symptoms of sensation. It is besides equally true, that the muscles frequently contract on stimulating the nerves, notwithstanding the creature has been a long time dead. So that the motion of the heart, and the power the nerves possess of contracting the muscles when stimulated, decay much later than the voluntary motions and sensations.

I have likewise observed, that opium, on being applied immediately to a nerve, not only does not deprive it of the faculty of contracting the muscles, but likewise does not destroy its natural sensibility; and

and it has been seen, that its effects are quicker when animals have been made to swallow it without their heart having been taken out, than when they have been deprived of that muscle. It seems to follow from all these particulars, that opium does not act immediately on the nerves, but has need of the circulation of the blood and humours, in exercising itself on animals.

I here conclude the principal consequences of my researches into opium, and I wish I had been able to have entered into a circumstantial detail of the experiments, in the way I have made them. I do not now regard the subject as exhausted, and am as far from such a belief, as I am from thinking that there is nothing to correct in my present work, and that nothing can be added to it. This very Supplement itself demonstrates the contrary, and if I could in any way retard the publication any longer, I might probably be myself able to add many new particulars, to see many others in a clearer light, and perhaps to correct several of them. I shall therefore lend a willing ear to criticisms, and to the objections that may be made against my work, and shall take a real pleasure in correcting and rendering it perfect for a new edition, should such an one ever become necessary. But I protest at the same time, that I will answer none of those pretended philosophers, who oppose words to facts, sophisms and cavils to experiments, possibilities to observations, and prejudices and scholastick errors to natural, direct, and luminous, consequences. Thus
I shall

I shall not think myself obliged to repeat my experiments, already repeated so many times, and to fancy myself in an error, on account of a few isolated experiments which some one may wish to oppose to me, simply because they are not exactly conformable to mine. A simple glance thrown on my work itself, will show how easy it is to be deceived in matters of experiment, even when many of them have already been uniform, and when one would the least expect the possibility of being led astray. My experiments, on the side of truth, exceed the number of six thousand, and the observations I have interspersed through the work are at least as numerous. I know very well that the questions I have proposed and examined are likewise very numerous, and that there may be some few in the number, as I have observed on a former occasion, that have not been discussed with as many experiments as were necessary. But in spite of all this, I firmly maintain, that a few experiments will not be sufficient to destroy the great number I have made, and varied in so many ways, and that like contradictions will not be capable of making me change my manner of thinking.

F I N I S.



I N D E X

TO THE

SECOND VOLUME.

A

<i>ACADEMY</i> Royal of Sciences at Paris ; experiments made by two of its members on the effects of common oil against the bite of the viper	- -	39
<i>Acids</i> , blended with the venom of the viper, do not deprive it of its deadly qualities	- -	8
—— they render the ticunas innocent	-	115
—— but are not a remedy when applied to wounds poi- soned by this substance	- -	117
<i>Air</i> is one of the most active principles in awakening irrita- bility	- - - -	142
<i>Albinus</i> ; his opinion of the structure of the brain	-	217
<i>Alkali volatile</i> , recommended as a specifick against the ve- nom of the viper, and particularly brought into vogue by "Jussieu	- - - -	2
—— experiments on its effects against the venom of the viper	- - -	3
—— does not seem to penetrate through the skin to the muscles	- - -	4
—— when blended with the venom of the viper it does not render it innocent	- -	6
—— it is not a specific against this poison in man		5

Alkalies

<i>Alkalies</i> cause no change in the ticunas	-	116
<i>Alexipharmick</i> of M. Zecmeyer	-	78
<i>Amber</i> observed with the microscope	-	304
<i>Amputation</i> ; its effects as they relate to the disease caused by the bite of the viper	-	16
———— very useful to guineapigs that have been venomd, provided it is made within six minutes after the bite	-	19
———— of the comb of fowls after they were bit, and its effects	-	24
<i>Animals</i> with cold blood; the effect of the ticunas on them		121, 139

B

<i>Bark Peruvian</i> ; is inefficacious in the bite of the viper		12
<i>Birds</i> ; the smaller ones die of the bite of the viper		34
<i>Bismuth</i> observed with the microscope	-	307
<i>Bite of the Viper</i> ; its effects on the skin	-	22
———— its effects are more violent in proportion as the animal is smaller	-	36
———— it is not so dangerous as was believed		46
———— it is in reality not mortal to man	-	47
<i>Blood</i> ; the change wrought in it by the ticunas	-	129
———— is not coagulattd in animals poisoned by the ticunas		134
<i>Bæerhaave</i> admits of nervous diseases	-	192
<i>Bones</i> ; their structure observed with the microscope		298
<i>Brain</i> ; its internal structure	-	241

C

<i>Canal</i> , discovery of a new one in the eye	-	311
<i>Cantharides</i> , the effects of these insects, either applied to a part bit by the viper, or taken internally	-	11
<i>Cats</i> , the larger ones make an obstinate resistance to the bite of the viper, but are at length killed by it	-	41
<i>Centipedes</i> ;		

<i>Centipedes</i> ; inieets, the bite of which is thought to be mortal	43
<i>Cherry-laurel Water</i> , is a very strong poison	143
———— its effects on wounds	146
———— its effects on the blood when injected into the vessels	149
———— does not act on the nerves	148
<i>Cherry-laurel</i> , the <i>empyreumatick oil</i> of, experiments on its effects	177
<i>Cherry-laurel</i> , the <i>essential oil</i> of, taken internally	169
———— is a very violent poison, both to warm and cold animals	171
———— effects it produces when ap- plied to wounds	173
———— experiments with this oil dried in the sun	175
<i>Cherry-laurel</i> , experiments on the water and oil of this plant	143 169
———— the different productions it yields by distil- lation	157
———— in what part of it the poisonous qualities seem to reside	179
<i>Cherry-laurel</i> , the <i>phlegm</i> of, its effects on animals	161
<i>Cleaby</i> , experiment he relates in his journals, on the subject of the <i>ticunas</i> swallowed by animals	103
<i>Cobalt</i> , observed with the microscope	307
<i>Cobras-stone</i> , Kempfer thought it of use against the bite of venomous animals	51
———— what it is thought to be	77
———— experiments made with artificial ones	84
———— on quadrupeds	86
<i>Common Oil</i> , has been supposed a specifick against the venom of the viper	38
———— experiments made on this subject by the Royal Society of London, and the Academy of Sciences at Paris	38 39
———— found inefficacious by two of the French Aca- demicians, and afterwards by Mead	39 40

I N D E X.

<i>Condamine M. de</i> , his account of the process employed in	
America, in making the ticunas	100
<i>Cotton</i> , observed with the microscope	303
<i>Cruikshanks Mr.</i> , his discovery of the reproduction of nerves	203
<i>Cylinders</i> , primitive winding of the animal body	272

D

<i>Diaphragm</i> , analysis of the tendinous part of it	261
<i>Dogs and Cats</i> , recover of the disease of the venom with a facility proportioned to the violence of their vomitings	10
<i>Dogs</i> , the effects of tartar emetick on these animals venommed by the viper	-
— amputation of their ears, after the latter had been bit by vipers	21
— small ones are easily killed by the venom of the viper, but those of a larger kind make a very powerful resistance to it	36

E

<i>Eau de luce</i> , employed against the bite of the viper	43
<i>Eels</i> , their gluten observed with the microscope	295
<i>Electricity</i> , tried against the bite of the viper	14
<i>Emetick</i> , seems to be of some efficacy against the bite of the viper	10
<i>Epidermis</i> , observed with the microscope	296
<i>Errours</i> , those are liable to who observe with the microscope	284

F

<i>Fat of the Viper</i> , received by Mead as a remedy against the bite of that animal	13
<i>Fat</i> , observed with the microscope	298
<i>Fomentation</i> ,	

<i>Fomentation</i> , is some relief to a part bit by the viper	10
<i>Fossils</i> , general microscopical observations on these substances	304
<i>Fowls</i> , amputation of their comb, &c. after they have been bit by the viper	24
—— the ticunas occasions no disease when applied to the comb	11
—— the effects of the venom of the viper on these animals not so violent as those on pigeons	35
—— singular disease that attacks them after they are bit in the comb	24
—— inefficacy of the ligature on these animals after they are bit	60
—— effects of the ticunas on them	111
<i>Frogs</i> , experiments made on them with the ticunas	121

G

<i>Gesner</i> , his opinion of the composition of the retina	247
<i>Gold</i> , observed with the microscope	306
<i>Guineapigs</i> , the smaller ones infallibly die when bit effect- ually, but many of the larger ones recover	35
—— utility of cutting off their legs after they have been bit by the viper	16
—— in what time it ought to be done, to be ef- fectual	19
—— effects of the ligature tried instead of ampu- tation	64
—— effects of the ticunas introduced into the wounds of these animals	112
—— ———— swallowed by them	105
—— effects of the spirit of the cherry-laurel swal- lowed by them	160
—— effects of the essential oil of this plant swallow- ed by them	170

H

<i>Hairs</i> , observed with the microscope	- -	293
<i>Haller</i> , his opinion of the structure of the nerves	-	
<i>Hartsborn burnt</i> , its effects against the bite of the viper		83
<i>Heart the</i> , is, of all others, the organ first affected in passions of the mind, and in nervous diseases	-	194
——— does not contract when the nerves that are sent to it are stimulated	- - -	<i>ibid</i>
<i>Hedysarum movens</i> , observations on this plant	-	302
<i>Hoffman</i> , affirms that all diseases derive their origin from the nerves	- - - -	186
<i>Hunter</i> , does not seem to allow a true reproduction in nerves that have been cut	- -	204

I

<i>Journal British</i> , what is related there on the subject of the ticunas taken internally	- - -	103
<i>Iris</i> , its motions are voluntary	- - -	189
<i>Irritability</i> , is destroyed by the ticunas	- -	142
——— and is awakened by the air	-	<i>ibid</i>
<i>Juice the milky, of the toxicodendron</i> , its effects on the human skin	- - - -	182
<i>Jussieu</i> , from the authority of Mead, believed the venom of the viper to be acid	- - -	2
——— reply to the cure wrought by the means of this pretended specifick	- - -	42
<i>Ivory</i> , observed with the microscope	- -	300

K

<i>Kempfer</i> , advises and practises the ligature in the bites of venomous snakes	- - -	49
<i>Kempfer</i> ,		

<i>Kempfer</i> , his treatment of people bitten	-	50
———— experiments made according to his plan	-	82

L

<i>Lead</i> , observed with the microscope	-	306
<i>Leeches</i> , employed against the bite of the viper	-	15
<i>Ligamentum ciliare</i> , the new canal of the eye formed of it		312
<i>Ligature</i> , its effects when tried on the legs of pigeons		27
———— when made immediately after the bite of the viper, and suffered to remain a certain time, seems to be an effectual remedy	- - -	32
———— was tried by <i>Kempfer</i>	-	50
———— experiments to determine its utility	-	53
———— on sparrows	-	54
———— on fowls	-	57
———— on guineapigs	-	60
———— on rabbits	-	65
———— tried in conjunction with scarifications	-	67
———— tried against the ticunas	-	118
<i>Lungs</i> , the change wrought in them by the ticunas	-	126

M

<i>Magnesia calcined</i> , observed with the microscope	-	305
<i>Malpighi</i> , his idea of the structure of the brain	-	241
<i>Marble white</i> , observed with the microscope	-	305
<i>Mascenai de</i> , cure he made with the volatile alkali on a per- son bit by a snake	- - -	43
<i>Meckel</i> , his definition of sneezing	-	188
<i>Membrane cellular</i> observations on	-	272
<i>Microscope</i> , the errors to which the observations made with this instrument are subject	-	284
<i>Monro Dr.</i> , his discoveries that relate to the nerves	218	222

<i>Monro Dr.</i> , his opinion of the primitive composition of several bodies	- - -	301 303 &c.
<i>Muscles</i> , action of the ticunas on them	- -	112
—— of animals killed by the ticunas become pale		126
—— their general primitive structure	-	263
—— reflections on their motions	- -	275
<i>Musgrave Dr.</i> , was of opinion that all diseases had their origin in the nerves	- - -	186

N

<i>Nails haman</i> , their primitive structure	- -	297
<i>Nerves</i> , microscopical examination of these parts	220	224
—— their structure may easily be mistaken	-	287
—— their elements	- -	228, &c.
—— reproduction of them after they are cut	-	203
—— an enquiry into their irritability	-	280
—— thoughts on their influence in diseases	-	186
—— effects of the ticunas applied to the surface of them		130
—— effects of the water of the cherry-laurel applied in the same way	- - -	148
<i>Nikel</i> , observed with the microscope	- -	307

O

<i>Oils</i> , blended with the venom of the viper, do not destroy its poisonous qualities	- - -	9
<i>Oil of Tobacco</i> , its effect on animals	- -	185
—— <i>Turpentine</i> , seems to possess a degree of efficacy, if the part bit by the viper is plunged in it	-	9
<i>Opium</i> , various opinions of its effects	- -	199
<i>Ox</i> , the new canal of the eye first discovered in the eye of this animal	- - -	311

P

<i>Pigeons</i> , are readily killed by the venom of the viper	-	35
—— the application of leeches is ineffectual to these animals after they have been bit by the viper	-	15
—— what may be expected from a well-managed ligature, in the cases of these animals bit by the viper		32
—— consequence of making the ligature before the part is bit	- - - -	29
—— not endangered by the amputation of the leg		20
—— effects of the ticunas swallowed by these animals		104
—— introduced into wounds purposely made	- - - -	110
—— effects of the spirit of cherry-laurel swallowed by these animals	- - - -	159, &c.
—— applied to mechanical wounds	- - - -	116
<i>Poisons</i> , have no immediate action on the nerves		136 152
<i>Poison Indian</i> , brought from the banks of the river of the Amazons	- - - -	98
—— of the arrows brought from the East-Indies	-	138
<i>Portenfield</i> , his ideas of the structure of the retina	-	247
<i>Pringle Sir John</i> , his opinion of nervous diseases	-	201
<i>Prochaska</i> , his observations on the structure of the nerves		217
—— of the muscles		264

R

<i>Rabbits</i> , the venom of the viper acts on them in proportion to their size	- - - -	35
—— effects of the ligature made on the limbs of these animals	- - - -	65
—— what may be expected from the cutting off of their ears, after the latter have been bit by the viper		20

<i>Rabbits</i> ; their skin is not penetrated by the effluvia of the volatile alkali	-	-	-	4
effects of the Ticunas on them, taken internally				103
introduced into mechanical wounds	-	-	-	107
applied to the surface of the nerves	-	-	-	131
introduced into the substance of the nerves	-	-	-	132
effects of the water of the cherry-laurel on these animals	-	-	-	144
on their nerves	-	-	-	148
effects of the spirit of the cherry-laurel swallowed by these animals	-	-	-	160 161
of the essential oil of the cherry-laurel				170
observations on the reproduction of their nerves after they have been cut	-	-	-	206
microscopical observations on the retina of the eyes of these animals	-	-	-	248
<i>Rattle-snake</i> ; an enquiry whether its bite is always mortal				44
<i>Redi</i> , does not speak of the ligature, in enumerating the remedies against the bite of the viper	-	-	-	48
<i>Remedies</i> ; a great variety of them employed by the country people against the bite of the viper				37
<i>Resin elastick</i> observed with the microscope	-			303
<i>Retina</i> ; its primitive structure	-	-		246
<i>Rose-leaf</i> observed with the microscope				303

S

<i>Salt common</i> , observed with the microscope	-			305
<i>Salts neutral</i> , united with the venom of the viper, do not deprive it of its poisonous qualities	-			9
<i>Sann. ni</i> ; the cure he effected with the volatile alkali on a person bit by the viper	-	-		43
<i>Scarifications</i> ;				

<i>Scarifications</i> ; an examination of their effects against the bite of the viper	- - -	12
_____ tried by Messrs. Geoffroy and Hanauld	- - -	40
_____ by Kempfer	- - -	50
_____ are more dangerous than useful	- - -	<i>ibid.</i>
_____ combined with the ligature are equally dangerous	- - -	67
<i>Silver</i> observed with the microscope	- - -	306
<i>Snake</i> ; is not acted on by the Ticunas	- - -	121
_____ when wounded with several poisoned arrows was simply benumbed by them	- - -	140
<i>Sneezing</i> , a voluntary motion	- - -	188
<i>Spar phosphorick</i> observed with the microscope	- - -	305
<i>Sparrows</i> , are recovered after they have been bit by the viper, by the immediate application of a ligature	- - -	54
<i>Sponges</i> ; their structure observed with the microscope	- - -	300
<i>Structure</i> , the primitive, of the animal body	- - -	215
<i>Suction</i> ; its effect on animals that have been bit by the viper	- - -	15
_____ practised in the bite of the rattle-snake	- - -	44
<i>Sympathies nervous</i> ; what opinion should be had of them	- - -	189

T

<i>Tartar stibiated</i> ; of some efficacy against the bite of the viper	- - -	10
<i>Tecmeyer</i> ; his alexipharmick	- - -	77
_____ his opinion of the nature of the venom of the viper	- - -	78
<i>Teeth human</i> ; their enamel viewed with a microscope	- - -	298
<i>Tendons</i> ; their structure	- - -	257
_____ do not receive nerves	- - -	262
<i>Theriaca</i> ; its effects in cases of the bite of the viper	- - -	13
_____ employed by the country people for this purpose	- - -	37
_____ recommended by Kempfer	- - -	50
<i>Ticunas</i> , the American poison ; an enquiry into its effects	- - -	96

<i>Ticunas</i> , the American poison; the vapours of it have been thought destructive	-	-	-	98
but are not so	-	-	-	99
characteristicks of this poi- son	-	-	-	101
it is innocent to the eyes	-	-	-	102
it is likewise thought to be innocent when swallowed	-	-	-	103
experiments made on this subject	-	-	-	<i>ibid.</i>
which prove it to be a poi- son when swallowed	-	-	-	105
its effects when introduced into the wounds of quadrupeds	-	-	-	106
into those of birds	-	-	-	109
the quantity of it required to kill an animal	-	-	-	110
has no effect on the comb of fowls	-	-	-	111
the time in which it pro- duces its effects on animals	-	-	-	118
has been deemed a poison to all species of animals	-	-	-	120
it is however innocent to adders and vipers	-	-	-	122
seems to excite a nervous disease	-	-	-	123
its effects on blood drawn from the vessels	-	-	-	124
has no action on the surface of the nerves	-	-	-	130
nor when it is introduced into their substance	-	-	-	132
its action is on the blood alone	-	-	-	134
<i>Tin</i> observed with the microscope	-	-	-	306
<i>Tobacco, oil of</i> , its effects on animals	-	-	-	185

<i>Torre della</i> ; his examination of the substance of the brain	216
————— of transpiration	294
<i>Toxicodendron</i> ; experiments made with this plant	182
————— effects of the milk of this plant	<i>ibid.</i>
————— its juice is innocent	<i>ibid.</i>
<i>Transpiration</i> ; microscopical examination of the materia perspirabilis	295
<i>Turpentine, oil of</i> , seems to possess a degree of efficacy, if the part bit by the viper is plunged in it	9
<i>Turtles</i> ; effects of the ticunas on these animals	140
————— of the oil of cherry-laurel on them	170

V

<i>Venom of the Viper</i> was thought by Tecmeyer to be acid	78
————— is a poison to all the species of animals with warm blood	40
————— acts with greater violence on small and delicate animals, than on the larger ones	35
————— is not innocent to man	41
————— causes no change in blood drawn from the vessels	125
————— not even in the shape of its globules	<i>ibid.</i>
————— is not rendered innocent by the volatile alkali	8
<i>Vinegar</i> does not counteract the deadly qualities of the ticunas	115
<i>Viper</i> is scarcely acted on by the ticunas	122
———— a single one not sufficient to kill a man	36
———— three of these animals did not kill a dog of nearly sixty pounds weight	<i>ibid.</i>
———— seems to have different degrees of activity in different climates	94

W

<i>Water</i> , when warm, is of some utility to the part bit by the viper - - - - -	10
<i>Wine of Burgundy</i> , given to people bit by the viper	40
<i>Wytt</i> , his opinion of nervous diseases -	197

Z

<i>Zinc</i> observed with the microscope - -	307
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E X P L A N A T I O N

OF THE

P L A T E S.

Explanation of the first Ten Figures of Plate the First.

Taken from Mead's Work on Poisons.

FIG. 1. offers the lateral view of the cranium and jaws of a viper. *a*, represents two venomous teeth on each side, fixed in a solid bone, by the mechanism that will be described hereafter. These solid bones at *b*, are articulated by ginglymus, as if they adhered to each of the processus zygomatici. By means of this articulation they have two motions ; by the first the teeth show themselves, and prepare to bite ; by the second they retire, and are carried within, bending themselves towards the root of the tongue, so as to approach the two jaws.

In Fig. 5. these teeth are seen on a larger scale.

These motions are produced by a gentle depression of the bone *c*, and in Fig. 5. *d*, which, fastened

to the bone *b*, above its articulation, forces it to unite with it, and to assist in these motions, by which it is either driven without or carried within; they are communicated to it, both by its connection with the lower jaw, and by virtue of the muscles which belong to it, and which are destined to this purpose.

Fig. 1. *f*, shows the lower jaw, and *e*, *d*, the two *axes* or points of support, by the means of which it performs the motions necessary to the devouring of its prey.

In Fig. 6. these two *axes*, or supporters, are seen at *a*, and *b*, as they serve to connect the lower jaw with the scapula and temporal bones.

To fully comprehend the mechanism the viper employs in swallowing its prey, it must be observed that the jaws, both upper and lower, *on the same side*, are capable of moving at the time the opposite ones remain fixed and motionless; so that both jaws on one side may be carried without, or brought within, whilst those on the opposite side either have contrary motions, or continue fixed and immoveable. Now these jaws are provided with small teeth, Fig. 1. *g*, and Fig. 5. *c*, which adhere very strongly to their superficies, and which, from the purpose to which they are applied, may be called hooks. It is in consequence of these alternate motions of retraction, that the prey is driven into the stomach.

The number of these hooks is more considerable in the upper than in the lower jaw.

Fig. 5. *c*, represents these teeth, or hooks, of the upper jaw; Fig. 6. *d*, those of the lower.

Fig.

Fig. 4. shows the upper part of the head. The sinciput at *a*, which in men is formed by the conjunction of the two parietal bones, is here formed of a single one; whilst the os frontis *b*, which is a single bone in men, is formed in this animal of two bones united by a future.

c, the antierour entry of the orbit of the eye, hollowed in the frontal bone.

d, the bones of the nose.

e, the maxillary bone, which in this animal is entire.

But before we leave the bones of this part, I must not forget to observe, that the venomous teeth differ from the others, not only on account of their size and motion, but in having very distinct qualities. In the first place, it must be remarked, that although two of them are found at each side, they are very rarely fastened with equal strength to the sockets that contain them. Sometimes the exterior tooth on each side is the loosest, and at other times this is the case with the interior ones. At others again, the internal on one side, and external on the other, are the loosest. When the teeth jut out or project, that which is the best fastened rises more than the looser one, which appears longer.

By weighing all these circumstances, and some others that remain to be mentioned, we see that the viper, in biting, only employs one of these teeth on each side. Nature has made this disposition, to the end that the action of one alone may suffice to convey into the animal the viper preys upon, all the

venom prepared on one side, and which is shed with as much efficacy to the reptile itself, as if both teeth had acted equally.

The tooth of the viper, in wounding, describes a segment of a circle, and this crooked shape, which makes it resemble in a degree the claws of a bird of prey, Fig. 1. *a*, and Fig. 5. *a*, gives it a much greater strength than it would otherwise have. But this shape prevents the tooth from disengaging itself, whence it sometimes happens that the animal preyed upon, in making efforts for its deliverance, plucks it out, particularly as the viper, perceiving itself dragged by a variety of motions, applies its tail to the earth, till it finds itself well fixed. If by this means it cannot preserve its tooth, the latter is broke in the weakest part of its articulation. Nature, to remedy this evil, has contrived that the tooth which was before loose, suddenly acquires a great degree of firmness, and that in the place of the one which fell, another, which is detached at pleasure, immediately succeeds: a tooth broken or plucked out, is soon supplied by the small rudiments of young teeth that are concealed in the socket betwixt the roots of the venomous teeth, and which, by degrees, at length become perfect.

In the rattle-snake I have remarked six teeth of this kind, growing on the same side. I shall not hazard any conjectures as to the cause of these auxiliary teeth falling into the empty socket; but from all that has been hitherto said, there is great reason to believe that their true use has been assigned

to them, since the preservation of these animals renders such a supply indispensably necessary.

The venomous teeth are hollow, from where they are fixed in the socket to the point. This hollow begins at the top of the orifice placed at the antierior part of the tooth, Fig. 2. *a*, and ends at some distance, near the summit *b*. The rest of the tooth is firm and solid, and cut like a tooth-pick.

Fig. 3. shows the hollow of this tooth cut in the middle.

The venom is prepared and supplied by a gland situated at each side of the cheeks. It is attached by a ligament (*a*, Fig. 9.) to the sinciput, where it proceeds to fasten itself to the occiput and lower jaw, by means of another ligament *b*. A strong white membrane which goes out of these ligaments, serves to bind it still faster, and to secure it from too great a compression, to which it would otherwise be subject, either by too great an accumulation of the venomous fluid, or by too violent a pressure made by the animal to expel this fluid. The excretory conduit *c*, is formed by a continuation of this same tunick. This conduit is carried from the gland to the cavity of the tooth, by means of the bag or sheath (Fig. 7. 8.) which encloses the teeth on each side.

f, Fig. 9. is a small white gland, the proximity of which to the teeth caused is to be taken, for the secretory organ of the venom, though it seems to be, nothing more than a lymphatick or salivary gland, and to be absolutely wanted in the rattle-snake. All
the

the muscles that concur towards the act of biting, are situated in the viper in such a way, that when they act they make a strong compression on the gland that contains the venom, and thus assist the ejaculation of the latter.

That which of all of them contributes the most to this ejaculation, is the muscle *d*, Fig. 9. After rising out of the lower jaw, it extends obliquely below the gland that contains the venom, till having passed betwixt the two ligaments, *a* and *b*, it is returned back upon the outer surface of the gland, and is strongly fastened to it in a direction parallel to its length, by the ligament *a*, which serves it as a tendon. This muscle may likewise assist in closing the jaws. Its greatest action however consists in making a strong compression on the venomous gland it so exactly surrounds; and this compression is made nearly in the way we employ in expressing the juice from an orange. The disposition of this muscle, (which extends over the whole surface of the gland, and takes the same direction as the excretory conduit) and the very end of this conduit (which appears tendinous, and terminates at the root of the teeth) gave rise to the opinion of this muscle serving for the retraction of the latter. But it is easy to be convinced of the contrary, by macerating a viper's head, the skin of which has been removed, in warm water, when the muscle separates easily, and shows the naked gland.

Fig. 7. presents an entire viper's head. At *a* are seen the two venomous teeth on each side, enclosed
as

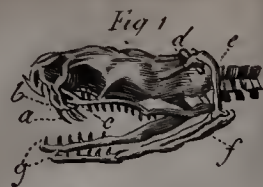


Fig. 10.



Fig 2



Fig 3



Fig 4



Fig 5

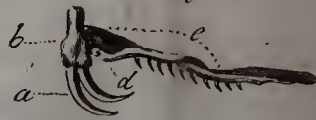


Fig 7

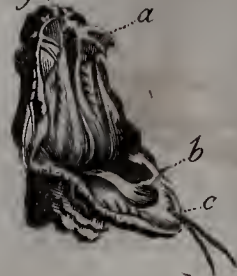


Fig 6

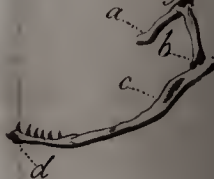


Fig. 9.



Fig. 8.



Fig. 10.



Fig. 11.

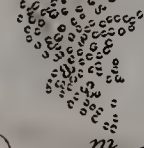


Fig. 9.

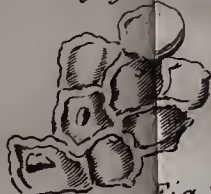


Fig. 8.



Fig. 1.

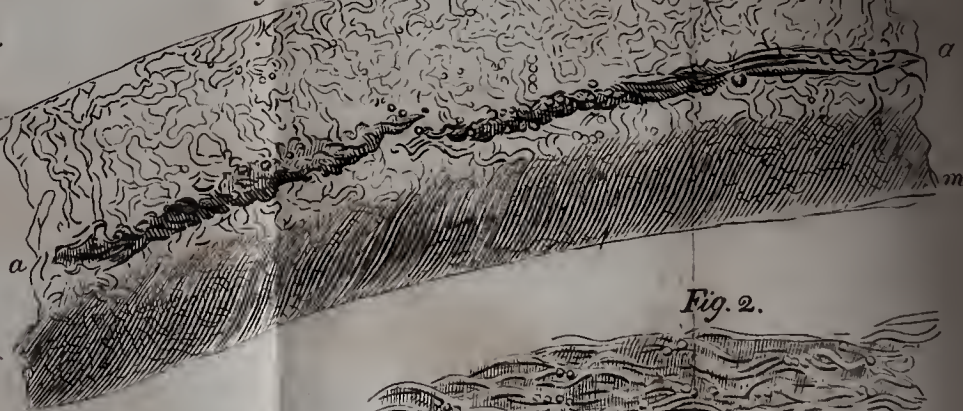


Fig. 2.

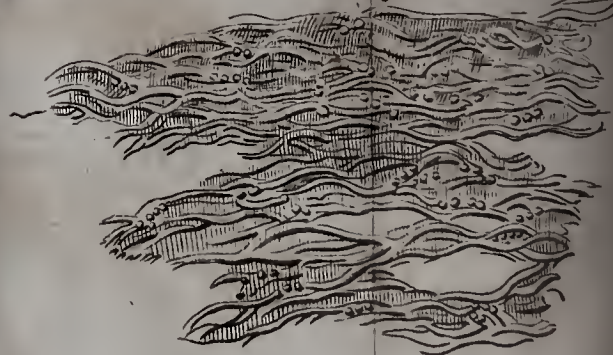


Fig. 4.

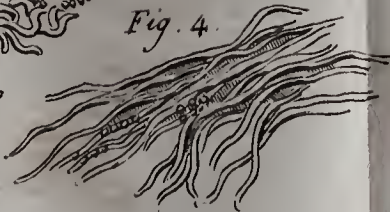


Fig. 5.

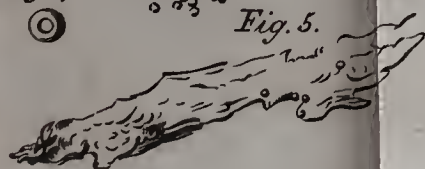
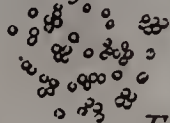


Fig. 7.



Fig. 6.



their proper sheath. The different degrees of erection and distension are readily observed.

b shows the entrance of the trachea placed in such a way, that it is as little as possible exposed to compression at the time of deglutition.

c shows the tongue of the viper, which serves it in sucking up the dew. It is this perhaps that places the auxiliary teeth in the empty sockets, as exigencies may require.

Fig. 8. shows the bag or sheath that encloses the two teeth. It is drawn large, that its fringed apertures *a*, may be the readier distinguished.

Continuation of the Explanation of Plate the First.

Fig. 1. *mm.* is a bit of hair, in which are seen, in the middle and internally, several brown spots; its whole surface seems to be covered with small winding cylinders, in some measure parallel to each other.

Fig. 2. represents a small portion of the same hair, which had been strongly compressed on a glass port-object, with an iron plate.

Fig. 3. is a fragment of the above figure, in which very small globules, detached from the winding cylinders, are distinguished.

Fig. 4. is another fragment of Fig. 2. which, having been steeped in water, takes the appearance of a transparent and irregular pellicle as it appears in Fig. 5.

Fig. 6. shows the globules of transpiration.

Fig. 7. is a globule of blood, observed with the same glass that was employed in the observation of Fig. 6.

Fig. 8. is a mass of the globules which form the gluten of the skin of eels. They appear like so many bladders filled with infinitely small globules.

Fig. 9. is the same mass of globules as in the preceding figure, but which have been a little dried. A small body is seen interiorly, situated in each globule in a different part.

Fig. 10. represents one of these globules of Fig. 9. which contained in its middle part a small body likewise spotted in the middle. *c.* which is one of the globules of blood, has been placed at the side of it, that their respective sizes may be compared.

P L A T E II.

Explanation of the Drawings marked with small Figures.

Fig. 1. represents the two canine teeth of the viper.

Fig. 2. shows the bag or sheath that covers them. *s s.* are the edges of this bag or sheath which has been divided with scissars. *n e.* are the two elliptical holes that are found at the basis of the tooth. *r a.* are two equally elliptical clefts, which are situated

Fig 1

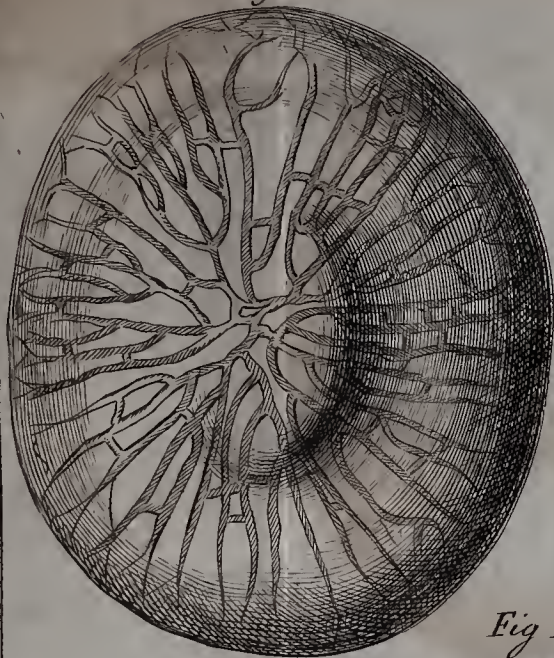


Fig II



Fig III

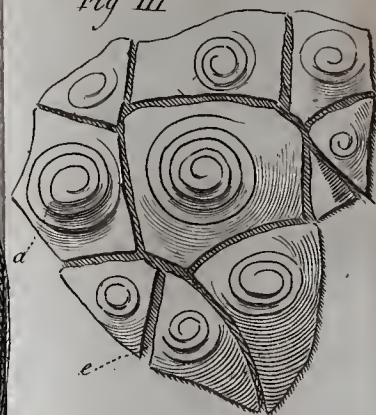


Fig IV

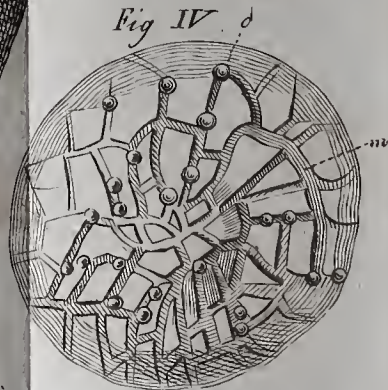


Fig 1



Fig. 6.

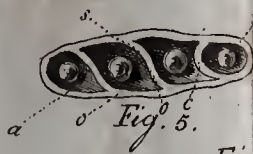
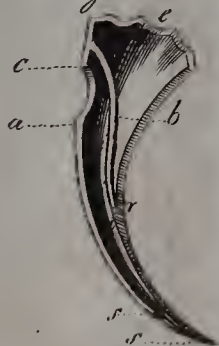


Fig 2

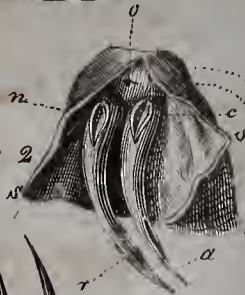


Fig 7

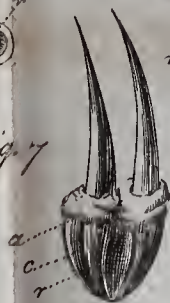
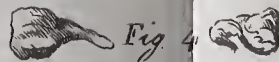


Fig 3



Fig 4





ated almost at the point of the same tooth. *m.* is the receptacle of venom : this receptacle opens in its upper part in a conduit, and this conduit proceeds to *o.* where it opens at the part in which the two teeth are fastened in their sockets.

Fig. 3. shows the same receptacle of venom, observed with a lens ; its shape appears to be nearly triangular.

Fig. 4. is this same receptacle in its natural size.

Fig. 5. is a transverse section of the above receptacle, which appears to be formed internally of several cavities filled with venom, and separated by partitions, *s o c.* ; the venom, as is indicated in the figure, flows out by drops.

Fig. 6. represents one of the canine teeth with all its cavities and apertures. *s s.* shows the elliptical cleft near the point of the tooth ; and *ca.* the hole found at the basis. *iii.* are a cavity of the tooth, closed at *r*, and only open at *e.* The transverse section of it seen at the side of the figure, and marked *m.* and the other small drawing at the side of the figure, *ar o d.* express another section of the same tooth, made in a direction with *a b.* of the figure itself.

Fig. 7. represents the gum in which the two canine teeth are fixed, and at their basis are seen six small teeth, which are not yet altogether formed, and which are destined to replace the canine teeth, when the viper chances to lose them. *a c r.* are three of these small teeth situated at the left side.

*Explanation of the Drawings of this Plate marked
with Numerals.*

Fig. II. represents a drop of the venom of the viper, such as it appears when it begins to dry a little on the microscope.

Fig. I. represents the same drop when entirely dry.

Fig. III. is a heap of several fragments of dried venom. The letter *a*. shows a singular cleft turned in a spiral form. The letter *c*. shows one of these clefts, which separates the fragments from each other.

Fig. IV. shows a drop of venom taken from the mouth of the viper, and left to dry on a bit of glass. At *o*. the small globules and knots are seen, which are nothing more than small air-bubbles. Letter *m*. shows one of the clefts that separates the fragments.

Explanation of Plate the Third.

Fig. I. represents a nerve seen with a lens that magnified six times. The letters *c c c*. show the white bands, which are of the same size, and situated at equal distances. The letters *o o, o o, o o*, are the intervals, the colour of which is not so pale; they are likewise of the same size, and equidistant.

Fig.



Fig I



Fig II



Fig III

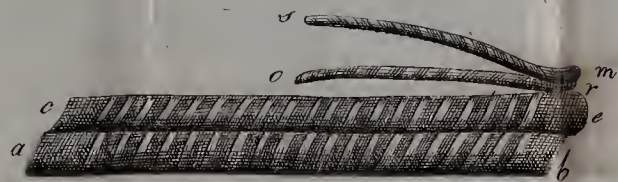


Fig IV

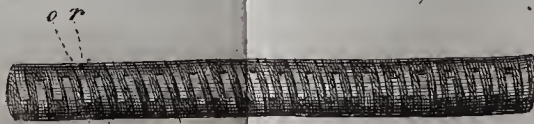


Fig V



Fig VI



Fig VII



Fig VIII

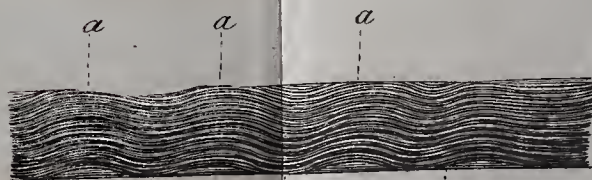


Fig IX

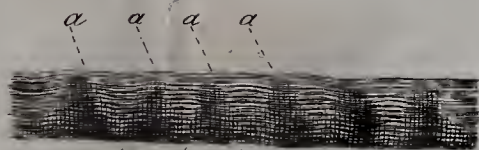


Fig X

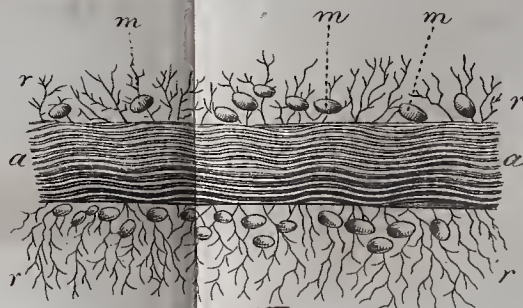


Fig XI

Fig. III. is a nerve enlarged about eight times by a microscope. Its bands are not so regular as those of the former figure, and in several places seem to cross each other.

Fig. II. is another nerve, the bands of which are more distinct, and approach each other in different parts with a degree of irregularity, but without crossing.

Fig. VI. represents a nerve, several of the bands of which approach, and others cross each other.

Fig. VII. is a nerve in which several of the bands cross each other at different angles.

Fig. V. shows a double range of bands in a nerve examined with a lens that magnified six times; the bands of the two ranges, *ar*, *oc*, are equally large, and are all situated at equal distances: they enter into each other, as the band *o*. is seen to enter into the band *a*. and the band *c*. into the band *r*.

The nerve of Fig. VIII. is composed of two nerves: *ra*, *ra*, show one of these nerves; and *ao*, *ao*, the other. The line *aa*. shows the union of the two nerves.

Fig. IV. shows a nerve separated into four, *ab*, *ce*, *or*, *sm*. In neither of these the bands either cross each other, or meet.

Fig. XI. represents a nerve enlarged by a very strong lens, and covered with its cellular membrane. *aa*. are the two extremities of the nerve. *mm*. are the oviform globules that are observed in the cel-

lular membrane. *r r r r.* are the filaments of this membrane, floating in water.

The figures IX. X. represent the way in which these bands appear and disappear, as they are observed with a greater or less degree of light, and with lens of different strengths. *c c c c.* are the white bands of the nerve of figure X. and *a a a a.* the dark spots. In turning the reflecting glass of the microscope the bands disappear, and the winding fibres of Fig. IX. are seen in their place. The bands *c c c.* of Fig. X. become the convex winding fibres, *c c c.* of Fig. IX.; and the opaque intervals *a a a a.* of Fig. X. take the appearance of the concave fibres *a a a a.* of Fig. IX.

Explanation of Plate the Fourth.

Fig. I. represents a primitive nervous cylinder which seems to have here and there on its coats several fragments of winding threads, and some round corpuscles in its interior part.

Fig. II. represents another cylinder, which seems filled with very small globular corpuscles, immersed in a gelatinous transparent humour.

Fig. III. represents three primitive nervous cylinders.

Fig. IV. represents a heap of primitive nervous cylinders. *o m.* is one of these cylinders entirely stripped of its external membrane. The cylinder represented by *n e.* is bare, except its extremity *n e.* which

Fig I



Fig III



Fig V



Fig VII



Fig IX

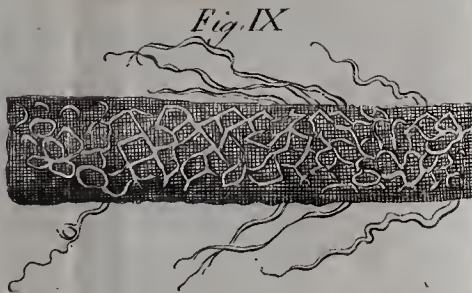


Fig XI

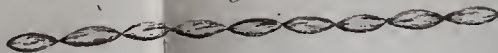


Fig II



Fig IV

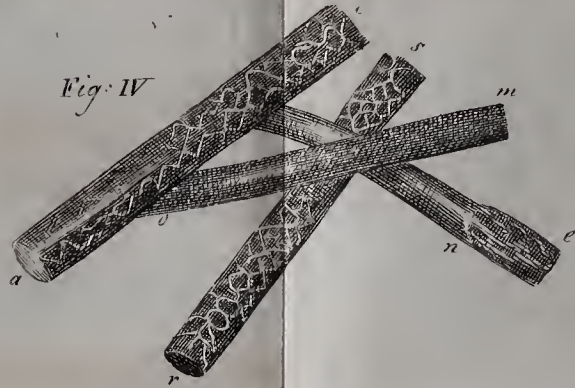


Fig VI

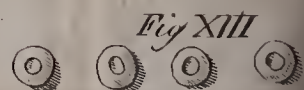
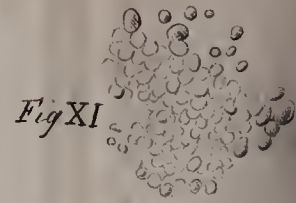
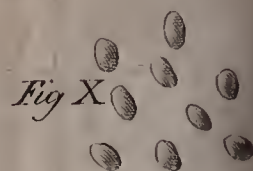
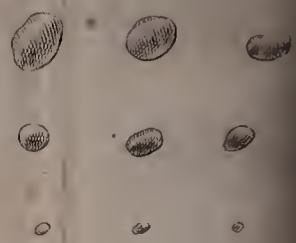
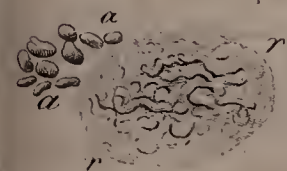
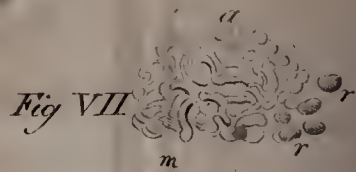
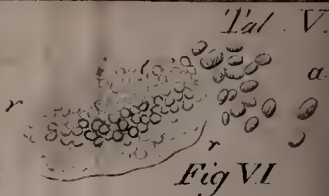
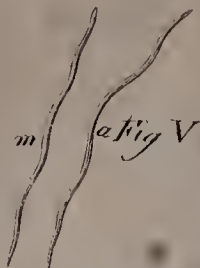
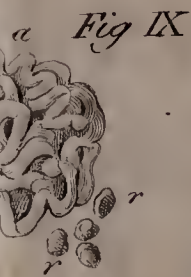
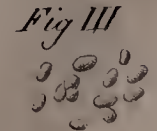
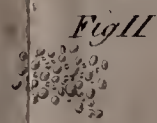
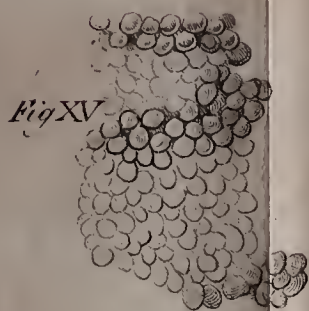
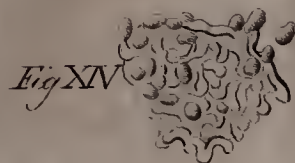


Fig VIII



Fig X





Tal V.

which is covered. The outer cylinder *a c.* is almost entirely covered with its membrane. The cylinder *r s.* is perfectly covered with its rugged membrane.

Fig. V. represents another of these primitive nervous cylinders.

Fig. VI. represents a primitive nervous cylinder, one half of which, *a c.* is formed of a transparent and uniform thread; and the other half, *m a.* is almost twice as large, less transparent, and irregular and rugged.

Fig. VII. represents a primitive nervous cylinder, in which *o r.* the thickest part, is covered with a cellular network, formed of very delicate filaments. The part *r s.* is stripped of this cellular net-work.

Fig. VIII. represents a primitive nervous cylinder covered with its exterior coat. This is composed, as it appears to be, of very small winding threads, which proceed for the length of the cylinder itself.

Fig. IX. represents a primitive nervous cylinder, covered with its external coat.

Figures X. and XI. represent two singular canals found in the substance of the brain.

P L A T E V.

Fig. I. represents several oviform bodies, found in the cellular covering of nerves.

Fig. II. represents very small corpuscles found in the medullary substance of the nerves.

Fig. III. shows the apparent size of the globules of the blood of a rabbit, observed with the same glass that was afterwards employed in Fig. IX.

Fig. IV. represents several winding cylinders of the cellular membrane of fat.

Fig. V. represents two threads, *m*, *a*, placed one at the side of the other, that their relative sizes may be the better seen. The thread *m*. belongs to the cellular membrane of fat, and the thread *a*. to the external cellular membrane of a nerve. They are both perfectly cylindrical, and pretty equal in size.

Fig. VI. *rr*. is a lamina cut from the cortical substance of the brain, and observed with a very strong lens. *ra*. are small round corpuscles, which appear to be filled with a gelatinous humour.

Fig. VII. *ma*. shows the windings and intestiniform circumvolutions that are seen in the cortical substance of the brain. *rr*. are the above corpuscles.

Fig. VIII. *rr*. represents a thin lamina of the medullary substance of the brain, which, observed with a microscope, seems to be formed of an heap of intestines *rr*.; at their side, at *aa*. are seen several corpuscles detached from the cortical substance.

Fig. IX. represents a part of the retina where it is not radiated. It seems to be composed of a very fine cellular membrane, interspersed with small globules, *rr*.

Fig.

Fig. X. shows these globules of the retina of their apparent size, relatively to that of the globules of blood of Fig. XIII.

Fig. XI. is another small portion of the retina, with the globules, and cellular membrane.

Fig. XII. represents the hollow of the eye, or the internal structure of the retina, of a rabbit. At *r r.* are seen the nervous rays, which proceed from the centre, and go on two opposite sides as far as the edges. The two opposite parts of the retina, *m m.* do not extend so far, and are not furnished with such considerable rays. These rays, or nervous fibrils, seem to be cut by knots or diaphragms placed at very small distances. The engraver has not been so happy in the execution of this figure as in the others. It is not possible to meet with an engraver who can express with the graving tool, all these little indeterminate strokes, which characterize the truth of the object, and which are not neglected by the person who at once delineates the object, and observes it with the microscope.

Fig. XIII. shows globules of blood observed with the lens employed in viewing the retina above. This is done that a comparison may be formed of their relative sizes.

Fig. XIV. represents a portion of the cellular net-work of the retina, which is simply a web of winding vessels, to which the globules are attached.

Fig. XV. represents a shred of the retina, after it has been a little macerated. Several of the globules that compose it appear to be detached; the
print

print of them however remains, and the hollow in which they were sunk.

Fig. XVI. shows several irregular bodies that were detached with the point of a needle from the medullary substance of Fig. IX.

P L A T E VI.

Fig. I. represents a tendon magnified only six times.

Fig. II. represents another tendon, likewise observed with a very weak lens.

Fig. III. represents a primitive tendinous fascia, which appears to be formed of several primitive and parallel tendinous threads. *a r.* are two of these threads detached from the others.

Fig. IV. represents another tendinous fascia stripped of its cellular membrane, and composed of primitive threads, *r r r.*

Fig. V. is a small portion of the cellular membrane of a tendinous fascia, which appears to be formed of several winding cylinders. *r r r r.* are the ends of these cylinders.

Fig. VI. represents four primitive fleshy fasciæ in contact with each other, and covered with their cellular membrane. The two marked *m m. s s.* have their small furrows in a circular form; in the other two, *a a. r r.* they are not so regular.

Fig.

Fig. I



Fig. III



Fig. II



Fig. V

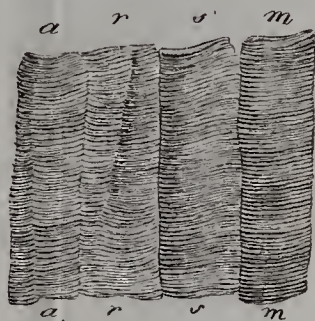
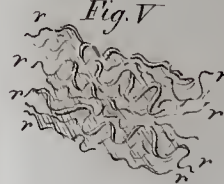


Fig. VI

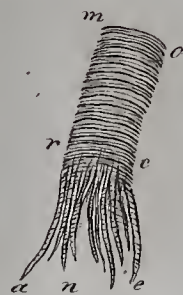


Fig. VII



Fig. VIII

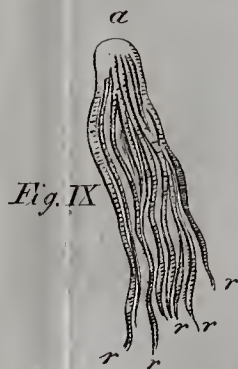


Fig. IX

Fig. IV



Fig. X



Fig. II

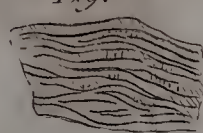


Fig. I



Fig. III

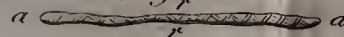


Fig. IV

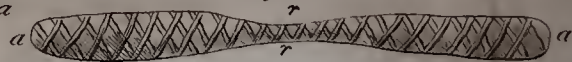


Fig. V

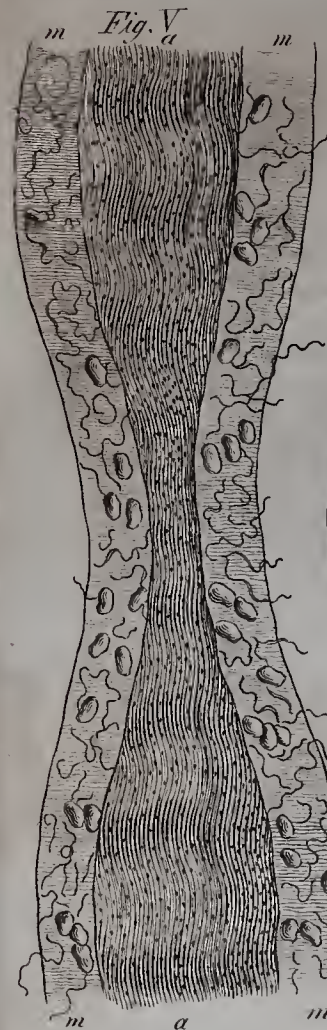


Fig. X

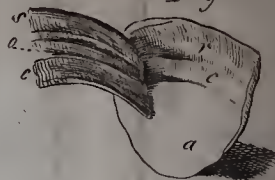


Fig. VIII



Fig. IX

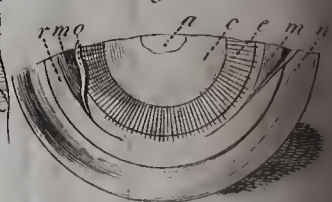


Fig. VI

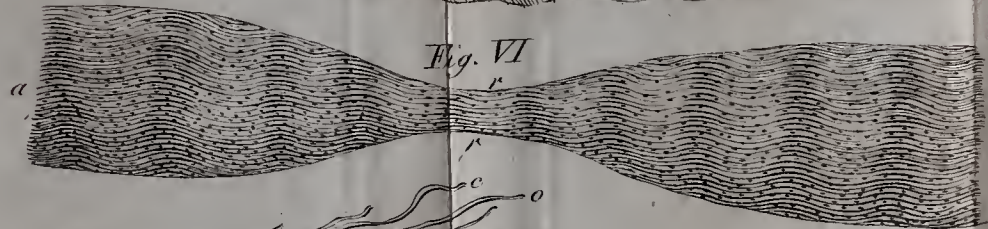


Fig. VII

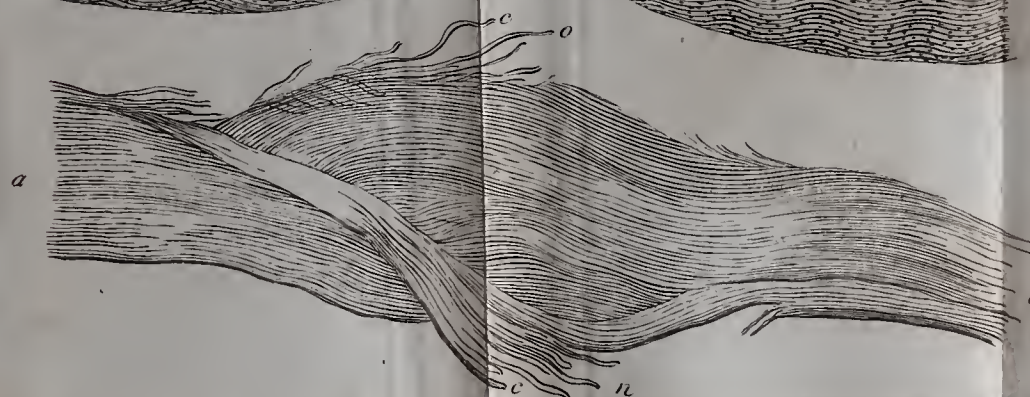


Fig. VII. represents a primitive fleshy fascia, partly covered with its cellular membrane.

a, e, are the primitive fleshy threads, separated and bare.

Fig. VII. represents a primitive fleshy fascia covered with its membrane.

Fig. IX. is the same fascia deprived of its membrane. Its threads are united at *a*, and are dispersed at the other end at *r, r, r*.

Fig. X. represents a small portion of the cellular membrane of the muscles, formed of winding threads *r, r, m, m*.

P L A T E VII.

Fig. I. represents a portion of the diaphragm of a rabbit. The letters *a, p, q, r*, show the fleshy part. *a, m, c, r*, the tendinous part. *n*, is the trunk of the nerve that goes to the diaphragm. *a, r*, are a vein. *f, f*, are branches of the nerve *n*. *a, r, y, y*, are branches of the vein. and *u, u, u, u*, are almost imperceptible ramification of the vein *a, r*.

Fig. II. represents a very small portion of the tendinous part of the diaphragm, observed with a very strong lens.

Fig. III. represents a nerve of the eighth pair of a rabbit, which had been divided twenty nine days before. It is drawn about twice as large as it naturally is. The letters *r, r*, show the part where it was reproduced.

Fig.

Fig. IV. is a repetition of Fig. III. magnified in a greater degree, that the spiral bands may be better seen.

The letters *n, n, n, n*, of these two figures, indicate a part of the reproduced nerve, where is seen a white annular spot.

Fig. V. represents the same nerve seen with a very strong lens. *a, a*, is the body of the nerve. *m, m, m, m*, the cellular membrane that covers its sides.

Fig. VI. is the same nerve stripped of its covering. The progress of the primitive nervous cylinders is seen in it, and the point of reunion or reproduction, where the diameter of the nerve, as well as that of the fibres, diminishes considerably, is marked *r, r*.

Fig. VII. is the same nerve, but a good deal torn with a needle, particularly at the part where it was reproduced, to determine the continuity of the primitive nervous cylinders. *a, a*, are the two ends of this nerve. *c, n, c, n*, several of the primitive cylinders that were torn.

Fig. VIII. represents the antierior half of the bulb of the ox's eye, observed at the concave side.

The letter *n*, points out the tunica sclerotis. *m*, the ciliary body, likewise called ligamentum ciliare. *e*, the processus ciliares. *c*, the uvea, and *a*, the pupil.

Fig. IX. is the half of Fig. VIII. in which the ciliary body, as well as the new canal *o*, is partly detached from the rest of the figure. *r*, is the tunica
nica



Fig 12

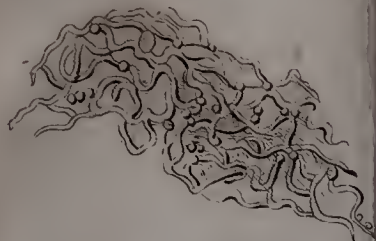


Fig.13.

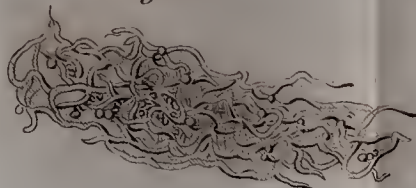


Fig.14.

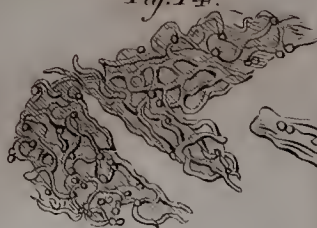


Fig 15

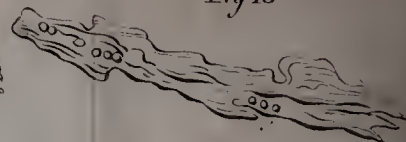


Fig 16

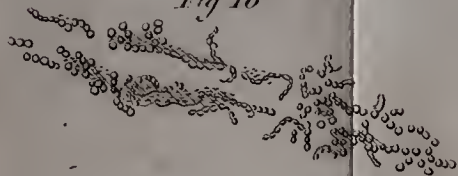


Fig 17.

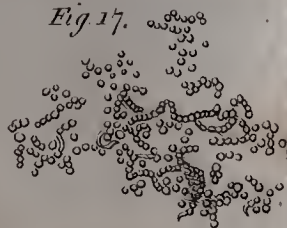


Fig. 20

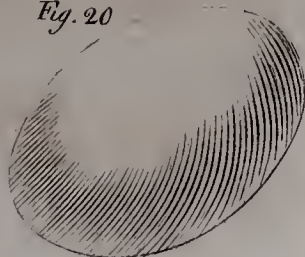


Fig.18.



Fig 22.



Fig. 23.



Fig.19.



Fig 21

nica sclerotis stripped of the tunica choroides. *c*, the small hollow in which the ciliary body, or ligament, is attached to the transparent cornea. The letters, *e*, *o*, *s*, indicate a membranous substance formed by the meeting of the tunica choroides *e*, the ciliary ligament *o*, and the uvea *s*. The letter *e*, shows the part of the ligament that is fastened in the hollow *c*.

P L A T E VIII.

Fig. 12. represents a very fine lamina of the epidermis. Very small globules are observed in different parts of it.

Fig. 13. represents another lamina of the epidermis, covered with water. It is not different from the first.

Fig. 14. represents a small bit of human nail observed dry.

Fig. 15. shows the same portion of nail, but steeped in water.

Fig. 16. shows the appearance of a small hollow made with a pin in a lamina of talc; both edges are interspersed with winding threads and globules.

Fig. 17. represents a pinch of hair-powder, or powdered starch, wetted, and afterwards observed with the microscope.

Fig. 18. shows the appearance of the above powder observed dry.

Fig.

Fig. 19. represents the vesicles of fat as they are seen betwixt the laminæ of the adipose membrane ; they are filled either with an oily or fatty humour, according to the animals to which they belong, and are covered with winding cylinders.

Fig. 20. shows one of the above vesicles deprived of its winding cylinders.

Fig. 21. represents a lamina, or rather a scraping, of ivory.

Fig. 22. represents a piece of cotton thread observed dry.

Fig. 23. shows the appearance of the same thread, steeped in water.

P L A T E IX.

Fig. 1. represents a cylinder of very pure gold, observed breadthwise.

Fig. 2. is the same cylinder, observed longitudinally.

Fig. 3. is a small bit of gold-leaf.

Fig. 4. shows four atoms of very pure silver filings.

Fig. 6. shows an atom of zinc, observed dry.

Fig. 7. represents several grains of calcined magnesia, the surface of which is covered with winding cylinders.

Fig. 8. shows an atom of bismuth, observed dry.

Fig. 9. represents two atoms of white marble.

Fig.

Fig. 1.



Fig. 2.

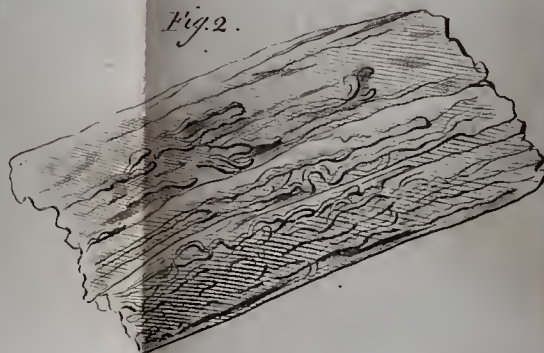


Fig. 3.

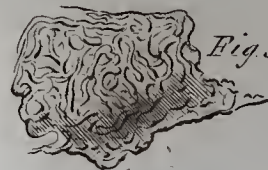


Fig. 4.



Fig. 5.

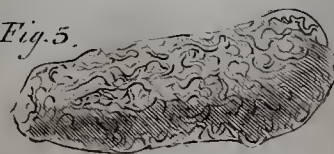


Fig. 6.

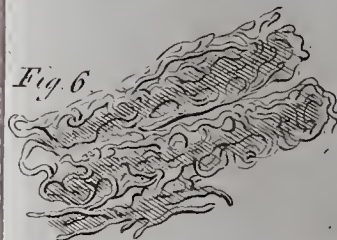


Fig. 12.

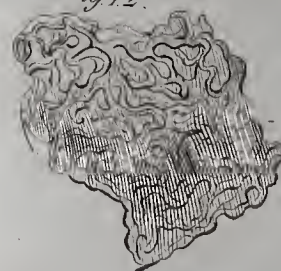


Fig. 8.

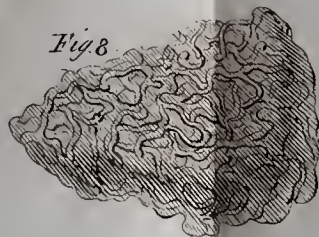


Fig. 10.



Fig. 7.



Fig. 9.

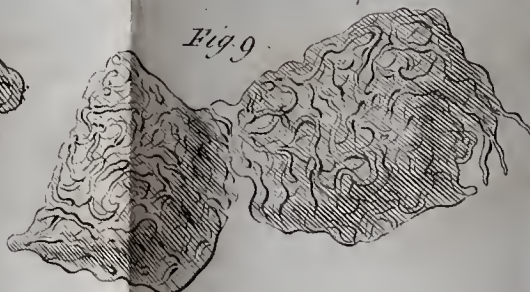


Fig. 11.



Fig. 7

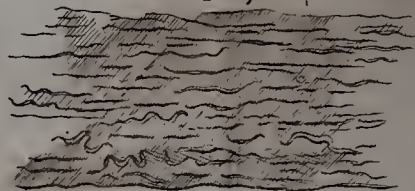


Fig. 2

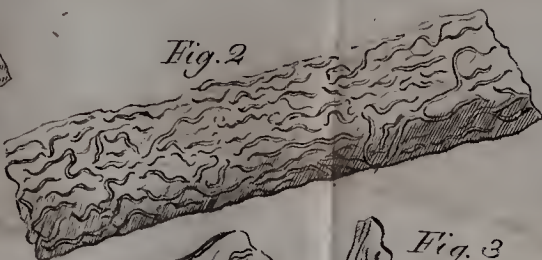


Fig. 1



Fig. 3

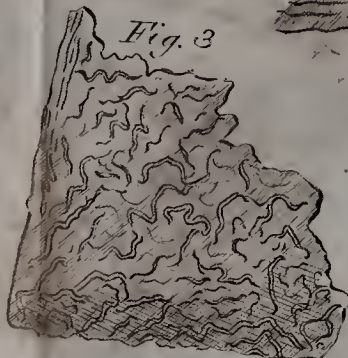


Fig. 4



Fig. 5

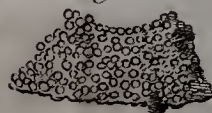


Fig. 8



Fig. 9



Fig. 10

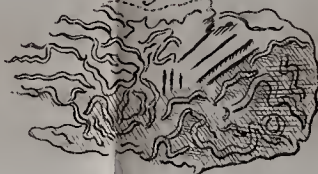


Fig. 11

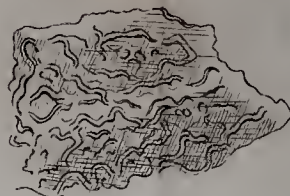


Fig. 12



Fig. 13



Fig. 14

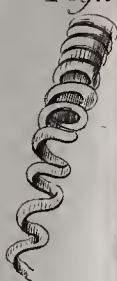


Fig. 15



Fig. 10. is an atom of heavy spar.

Fig. 11. is a fragment of phosphorick spar.

Fig. 12. is an atom of nikel.

P L A T E X.

Fig. 1. represents one of the branches, or branched cylinders, of which sponges are formed.

Fig. 2. represents a very small fragment of elastic resin.

Fig. 3. is a grain of common salt.

Fig. 4. is an atom of silver, in which the winding threads are seen interspersed here and there as usual.

Fig. 5. shows another small bit of silver, in which, instead of the winding threads, small shining grains are observed.

Fig. 6. is likewise another small bit of silver, which appears to be formed of points and pyramids.

Fig. 7. represents a small bit of tin, likewise furnished with the usual winding threads.

Fig. 8. is a bit of antimony.

Fig. 9. is an atom of cobalt.

Fig. 10. is a very fine lamina of lead, covered with the usual winding threads.

Fig. 11. is a bit of copper, which, like the other bodies, shows on its surface the winding threads.

Fig. 12. is a small fragment of rose-leaf, partly torn with the point of a knife.

Fig. 13. shows a pipe, formed by a spiral band, found in the stalks of the leaves of the *hedyfarum movens*.

Fig. 14. is the same pipe, partly stripped at its inferior end.

Fig. 15. is a fragment of amber, which like all other bodies, appears to be covered with winding cylinders.

